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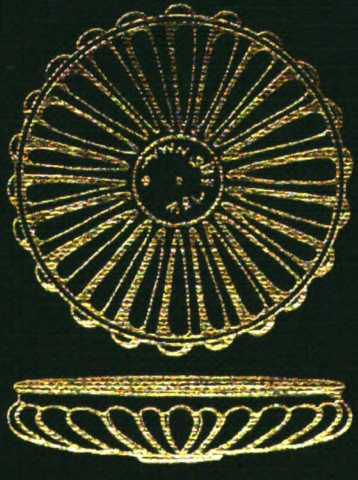
THE ALPHABET AT THE TURN OF THE MILLENNIUM

THE WEST SEMITIC ALPHABET *CA.* 1150-850 BCE
THE ANTIQUITY OF THE ARABIAN, GREEK AND PHRYGIAN ALPHABETS

BENJAMIN SASS



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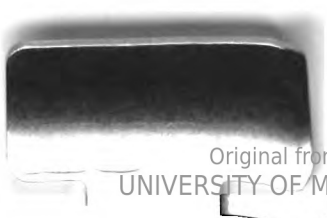

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EMERY AND CLAIRE YASS PUBLICATIONS IN ARCHAEOLOGY

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Cover Illustration: Ninth-century BCE bronze bowl from Kefar Veradim with incised Proto-Canaanite inscription (adapted from Fig. 12 by Rodica Penchas).

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CONTENTS

ACKNOWLEDGEMENTS 9

INTRODUCTION 11

CHAPTER 1. THE WEST SEMITIC ALPHABET *CA.* 1150–850 BCE 13

1.1 Introduction 13

1.2 From Proto-Canaanite to Phoenician *ca.* 1000 BCE: The 'Traditional' Chronology and Its Problems 14

1.3 From Proto-Canaanite to Phoenician–Aramaic and Hebrew *ca.* 900–850 BCE 16

1.3.1 Chronology of the Archaic Byblos Inscriptions 1: Non-palaeographic Criteria 16

1.3.1.1 Abibaal and Elibaal 16

1.3.1.2 Ahiram 17

1.3.2 Chronology of the Archaic Byblos Inscriptions 2: Palaeographic Criteria 22

1.3.2.1 Introduction 22

1.3.2.2 Archaizing and Non-archaizing Features in the Script of the Byblos Inscriptions 28

1.3.2.3 'Eccentric' Features in the Script of the Byblos Inscriptions 31

1.3.2.4 The Bordreuil Champlevé Inscription, and an Interim Balance of the Byblos Archaism 32

1.3.3 Chronology of the Latest Proto-Canaanite Phase 34

1.3.3.1 Two Inscribed Bronze Bowls and Their Archaeological Contexts 34

1.3.3.2 The Tell Fekheryeh Script 40

1.3.3.3 The Arrowheads and the Last Proto-Canaanite Phase (Tenth–Ninth Centuries)—Conclusions 43

1.3.3.4 The Antecedents: The Penultimate Proto-Canaanite Phase (Twelfth–Tenth Centuries) 44

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1.3.4 Chronology of the Archaic Byblos Inscriptions 3: The Extent of the Archaism 45

1.3.4.1 Relative Chronology: Palaeography and the Sequence of the Byblos Kings 46

1.3.4.2 Absolute Chronology: The Beginning and End of the Byblos Sequence 48

1.3.4.3 The Byblos (and Fekheryeh) Script: A Possible Source 50

1.4 The West Semitic Alphabet *ca.* 1150–850 BCE and Its Cultural and Political Background 50

1.4.1 West Semitic Writing *ca.* 1150–850 BCE 50

1.4.1.1 Introduction 50

1.4.1.2 New West Semitic Chancelleries and the Phoenician–Aramaic and Hebrew Scripts 51

1.4.1.3 The Galloping 800s: From Proto-Canaanite Via Byblos to Dan and Mesha in Less Than a Century 52

1.4.1.4 The Linear Alphabet in Phoenicia and Syria ca. *1150–850 BCE, and Iron I Antecedents* 53

1.4.1.5 Elusive Beginnings: The Phoenician–Aramaic (and Hebrew) Script ca. *900–850 BCE* 55

1.4.2 The Social and Political Background of the West Semitic Alphabetic Developments 61

1.4.2.1 The Northern Levant 1: Aramaean Statehood in the Eleventh–Tenth Centuries? 61

1.4.2.2 The Northern Levant 2: Aramaeans, Luwians and Assyrians in the Ninth Century 64

1.4.2.3 Phoenicia and the Southern Levant: An Iron I Prelude 66

1.4.2.4 Conclusions: The Levant and Its Alphabets, Chiefly ca. *900–850 BCE* 67

1.4.2.5 Beyond the Levant: The Semitic Alphabet and the Phoenician Westward Expansion 68

1.5 The West Semitic Alphabet *ca.* 1150–850 BCE—Synopsis 68

1.5.1 Main Points Recapitulated 69

1.5.2 The Chronology and Its Implications—An Overview 70

Digitized by Google

Original from UNIVERSITY OF MICHIGAN

EXCURSUS A. SIX NOTES ON AHIRAM'S SARCOPHAGUS, ITS RELIEFS, AND THEIR TIME 75

EXCURSUS B. NON-HEBREW INSCRIPTIONS OF THE NINTH CENTURY 83

EXCURSUS C. NINTH-CENTURY HAZOR AND THE EARLIEST HEBREW INSCRIPTIONS 85

EXCURSUS D. DATING THE AIN DARA SCULPTURE AGAINST ITS SYRO–HITTITE BACKGROUND 89

EXCURSUS E. ON THE CHRONOLOGY OF GOZAN 93

CHAPTER 2. THE ANTIQUITY OF THE ARABIAN ALPHABET 96

2.1 RECENT EXCAVATIONS IN YEMEN AND THEIR INSCRIBED FINDS 96

2.1.1 Yalâ 97

2.1.2 Raybûn 100

2.1.3 Hajar Ṣurbân 104

2.1.4 Al-Midaman 105

2.1.5 Early Monumental Inscriptions 106

2.1.6 Comments on the Published Results of the Excavations (Sections 2.1.1–2.1.5) 106

2.2 THE ADOPTION OF THE ALPHABET IN ARABIA 113

2.2.1 Non-epigraphic Evidence 113

2.2.1.1 Archaeological Dating of Excavated Inscriptions, Stylistic Dating of Inscribed Seals 113

2.2.1.2 The General South Arabian Picture and the Assyrian Synchronism 115

2.2.1.3 Fertile Crescent Picture 118

2.2.2 Epigraphic Evidence 119

2.2.2.1 The West Semitic Prototype and the Time of the Adoption 119

2.2.2.2 The hlḥ *Alphabetic Order and the Time of the Adoption* 122

2.2.2.3 The Early South Arabian Cursive and the South and North Arabian Stone Inscriptions 124

2.3 THE ANTIQUITY OF THE ARABIAN ALPHABET—SYNOPSIS 128

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Original from UNIVERSITY OF MICHIGAN

CHAPTER 3. THE ANTIQUITY OF THE GREEK AND PHRYGIAN ALPHABETS 133

3.1 Introduction 133

3.2 The Adoption of the Alphabet By the Greeks 134

3.2.1 The Antiquity of the Greek Alphabet—Selected Studies 1933–2000 134

3.2.2 The Mother Script and Locale of the Transmission—Selected Studies 138

3.2.3 The Antiquity of the Greek Alphabet 140

3.3 Phrygian Precedence? 146

3.4 The Antiquity of the Phrygian and Greek Alphabets—Synopsis 149

Excursus F. On Protogeometric and Geometric Chronology 153

Excursus G. The Osteria dell'Osa graffito 155

AFTERWORD 157

LIST OF TABLES AND FIGURES 159

BIBLIOGRAPHY AND ABBREVIATIONS 161

ADDENDA 183

INDICES 186

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INTRODUCTION

This book addresses the epigraphical and archaeological aspects of the West Semitic, Arabian, Phrygian and Greek alphabets *ca.* 1150–750 BCE,[1] and explores the processes and background which brought about the transition from the Proto-Canaanite to the Phoenician–Aramaic and Hebrew scripts in the first half of the ninth century, to the genesis of the Arabian alphabet at about the same time, and to the transmission of the alphabet to the Phrygians and Greeks *ca.* one hundred years later (Fig. 1).[2]

New archaeological discoveries have transformed our perception of southern Arabia and Phrygia during these periods. And concerning the Levant, and Palestine in particular, *ca.* 1150–850, while their chronology and social and political configuration are viewed quite differently today from the way they were viewed ten or fifteen years ago, the perception of the alphabet in the scholarly literature has for the most part remained unaltered. The following chapters seek to integrate alphabetic writing into this changed picture.

Issues with less direct bearing on the West Semitic alphabet *ca.* 1150–850 are dealt with in Excursuses A–E to Chapter 1: the reliefs on Ahiram's sarcophagus, Phoenician–Aramaic and Hebrew inscriptions of the ninth century, and the chronology of Ain Dara and Tell Halaf. Excursuses F and G to Chapter 3 deal with the absolute chronology of Greece and with the Osteria dell'Osa graffito.

---

1 The discussion of each individual alphabet covers only part of the said time range.

2 As Fig. 1 cannot express in full the range and uncertainties of the chronology, the relevant sections in Chapters 1, 2 and 3, and note 87 on 'Phoenician–Aramaic', must be consulted.

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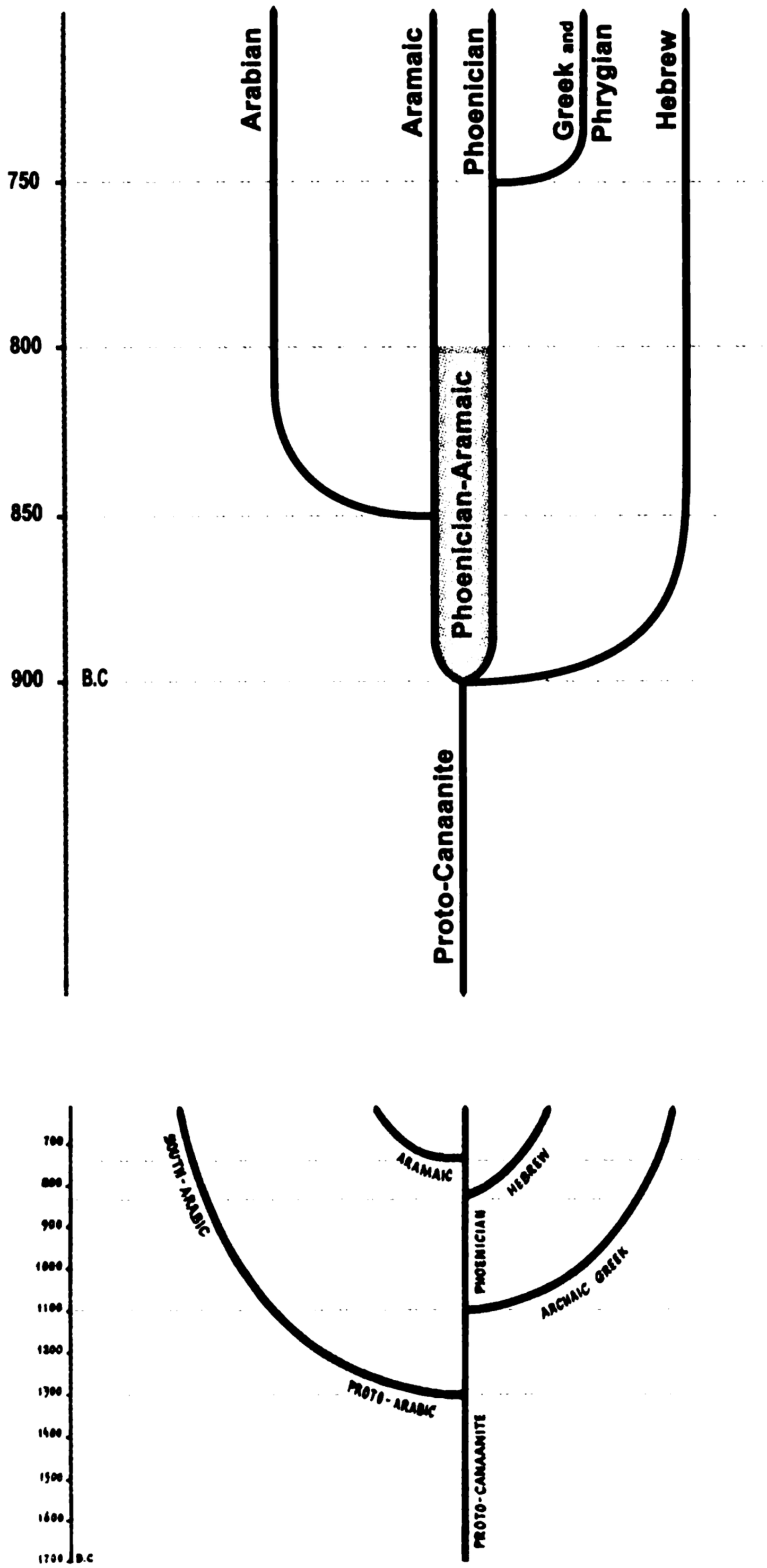

Fig. 1. Schematic family tree of the early alphabetic scripts (and below, for comparison, the pertinent segment of the tree in Naveh 1982: 10).

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CHAPTER 1

THE WEST SEMITIC ALPHABET *CA.* 1150–850 BCE

1.1 INTRODUCTION

As a result of new discoveries and insights, key aspects of the West Semitic alphabet *ca.* 1150–850 BCE are seen differently today than they were nearly twenty years ago, when they were dealt with in *Genesis of the alphabet* (Sass 1985, 1988). The aim of Chapter 1 is to present this entirely changed picture, with an emphasis on the early first millennium. Indeed, that time-span pertained to my older work mostly indirectly,[3] but even so my impression in 1985 and 1988 was that the 'traditional' absolute chronology of the pivotal archaic Byblos inscriptions was riddled with contradictions, stemming from too high a setting. If, despite these misgivings, I tied the lower end of the Proto-Canaanite series to the long-accepted 1000 BCE date for Ahiram, it was because I could not figure an immediate way out of the chronological impasse.[4]

Occupied as I was then with second-millennium epigraphy, I chose not to delve deeply into the multi-faceted, very different world of the early first millennium West Semitic inscriptions. Besides, doing this properly would also have entailed a review of the archaeology of Palestine, or rather of the entire Levant, at the turn of the millennium, a task far too removed from my work at that time.

---

3 Via the alphabets of Arabia and Greece, and a few West Semitic inscriptions and phenomena, all of which the 'traditional' chronology dated—mistakenly I thought—to the late second millennium (see note 8).

4 "[T]here is definitely good reason for a re-evaluation of these [the archaic royal Byblos] inscriptions.... In any event, my use of the date 1000 B.C. for Ahiram's sarcophagus is purely for the sake of convenience. If future research should elicit a different date, this would have some effect on the dates given here for the later Proto-Canaanite...inscriptions..." (1988: 154; similarly *ibid.*: 77, 88–95, 149, 153).

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In the years that followed, my doubts further increased regarding the soundness of the 1000 BCE dating of Ahiram, hence of the final segment of my sequence, *ca.* 1150 BCE onwards.[5]

Now I propose an overall change in the underlying perceptions and chronology of this segment, assigning the latest Proto-Canaanite inscriptions as well as the archaic Byblos inscriptions to after the tenth century,[6] and anchoring them this time to the more accurately-dated monumental inscriptions of the second half of the ninth century.

1.2 FROM PROTO-CANAANITE TO PHOENICIAN *CA.* 1000 BCE: THE 'TRADITIONAL' CHRONOLOGY AND ITS PROBLEMS

Let me begin with the 'traditional' chronology and the reasons I regard it as unworkable. The early monumental inscriptions from Byblos were attributed to the tenth century, so that the latest Proto-Canaanite inscriptions, less developed palaeographically though undatable archaeologically—primarily the arrowheads—were quite naturally placed in the eleventh century. Authors in the 1940s (see p. 17) devised the Byblos framework, visionary for its time, when the inscriptions at odds with this framework were not yet known.[7] Refining the scheme and filling it in with new discoveries, their successors continued to adhere to it even though, from the 1970s onward, contradictions began to crop up. The scheme was evidently considered untouchable, for the inscriptions and phenomena that called it into question by appearing too young were revised upward without fail. Thus, the earliest examples of Aramaic and Greek writing, and the Phoenician westward expansion, were placed in the eleventh century instead of some 250 years later as suggested by various pieces of evidence.[8] And whereas there were also eminent epigraphists who maintained the post-tenth-century date of these three phenomena, no one concluded that this called for a considerable lowering of the 1000 BCE dating of Ahiram and of the absolute position of the Byblos and the late Proto-Canaanite inscriptions in general (more in note 33).

A striking feature of the West Semitic alphabet *ca.* 1150–850 BCE is its 'floating'

---

5 The absolute chronology of the Proto-Canaanite inscriptions *ca.* 1300–1150 BCE, on the other hand, founded as it is on archaeological context (Sass 1988: 58–72; 151–152), seems to me to be in no need of a general revision.

6 This is not the first time that a lower dating for the archaic Byblos inscriptions is suggested; see p. 16 on Wallenfels 1983.

7 In the 1940s dating Ahiram *ca.* 1000 BCE was considered a substantial lowering—from the then preferred thirteenth century—see pp. 77–78.

8 *Aramaic alphabet*: Fekheryeh – pp. 40–43; *Greek alphabet*: Chapter 3; *Phoenician westward expansion*: Tekke bowl – pp. 34–36, Nora fragment – note 131, general – p. 68.

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chronology—the near-lack of absolutely dated texts. No inscription names a datable person or event and, save two inscribed objects, the Tekke and Kefar Veradim bowls (pp. 34–39), published in 1979 and 2002 respectively, none originated in a clear archaeological context. The 'Izbet Ṣarṭah ostracon and Raddana handle come from Iron I sites, but the former inscription is unstratified and the latter a surface find. The handle, moreover, is also otherwise problematic—see pp. 44–45.

I turn first to the preceding and succeeding periods: The latest Late Bronze Age Proto-Canaanite inscriptions are safely assigned to the twelfth century by their archaeological context (cf. note 5). The next texts with a secure date are ten inscriptions (p. 83), attributable to the second half of the ninth century by the mention of rulers, who in turn are linked directly or indirectly with the absolute chronology of Assyria. Many of the inscriptions that are palaeographically in between, i.e., more developed than in the Late Bronze Age but less than the Mesha stele, etc. (chiefly the arrowheads and the archaic Byblos inscriptions mentioned below and in note 10), can at most be arranged by their letter shapes in a relative sequence. But *until twenty-five years ago not one Levantine alphabetic inscription from the mid-twelfth to the mid-ninth century could be assigned an absolute date within this 300-year period.*

It follows that the high datings that dominated Levantine palaeography of the turn of the second millennium were merely the highest possible from a range. That the scheme could be devised at all rested on this ambiguity—the total absence until 1979, when the Tekke bowl was published, of inscriptions with a fixed date.

The reader may object: Even prior to 1979 not all dates floated. At least two absolutely dated inscriptions were recognized long before. These are the Phoenician dedications, *KAI* 5 and *KAI* 6, added by Byblos rulers Abibaal and Elibaal to statues, found at the site, of Pharaohs Sheshonq I and Osorkon I.[9] This leads me to the five royal stone inscriptions from Byblos, traditionally dated to the tenth century, among which another, that on the Ahiram sarcophagus (*KAI* 1), may likewise be datable by non-palaeographical means.[10]

---

[9] 945–924 and 924–889 BCE respectively according to Kitchen's chronology. Whereas there is probably not much room for error, these dates seem to be more precise than the Egyptian evidence for the early 22nd dynasty allows and should preferably be replaced by a range of a few decades; for Sheshonq say some time in the second half of the tenth century. The two statues were discovered before the excavations, in unknown circumstances. Additional fragments of Osorkon's statue came to light in the excavations. See Montet 1928: 49–54; KAI II: 7–8.

[10] The remaining royal Byblian inscriptions belong to Kings Yahimilk (KAI 4) and Šipiṭbaal (KAI 7). KAI 2 is the Ahiram graffito. For the unprovenanced Bordreuil champlevé inscription see pp. 32–33. Early inscriptions on other materials are the bronze spatula (KAI 3) and two texts with more advanced script—cone B (Sass 1988: 85–86) and the Abdô sherd (KAI 8), all three unstratified and also undatable by object typology. For Byblos cone A see p. 44. Colour photographs of some of these inscriptions are found in Matoïan 1998: 125–131.

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But before moving on, I would like to express my indebtedness to Ronald Wallenfels who, in an abridged version (1983) of his M.A. thesis (Columbia University, 1982), dated the Byblos inscriptions to the eighth and seventh centuries. This was his choice, very carefully substantiated, from a wider time-span—the ninth–seventh centuries. Whereas I may disagree with one or another detail of Wallenfels' multi-faceted paper, I take his work to be more judicious, balanced and thorough than anything written on the subject in decades. Indeed, much of what is said here on Byblos was already touched upon by Wallenfels in some way. The differences between our works are pronounced as well: The present chapter is written in the wake of two developments of the past decade—the introduction of the corrected, lower chronology of Palestinian archaeology, and the discovery of the Kefar Veradim bowl. It deals with the more general issue of the transition from the Proto-Canaanite to the 'national' alphabets in the context of the political transformation of the Levant *ca.* 900–850 BCE, and advocates that the script of the Byblos inscriptions (among others cut in stone) is artificial or archaizing to an extent that precludes a palaeographical analysis as in ink inscriptions. This implies in turn that the Byblos inscriptions cannot be arranged in an inner chronological order based on the development of the script.

1.3 FROM PROTO-CANAANITE TO PHOENICIAN–ARAMAIC AND HEBREW *CA.* 900–850 BCE

This section addresses the Byblos inscriptions and their relationship to the preceding and succeeding texts.

1.3.1 CHRONOLOGY OF THE ARCHAIC BYBLOS INSCRIPTIONS 1: NON-PALAEOGRAPHIC CRITERIA

1.3.1.1 Abibaal and Elibaal

Obviously only a *terminus post quem* for the Phoenician script is provided by the names of the two pharaohs. This was observed nearly sixty years ago by Benjamin Mazar (1946: 174, 178 = 1986: 240, 244):

> What is clear is that Elibaal…commanded the votive inscription to be carved on the statue…some time—perhaps even quite a long time—after the statue with its cartouche [of Osorkon I] had been erected…. It is…very likely that Abibaal made use of the statue several generations after the campaign of Shishak…. For Elibaal's writing we have only a terminus a quo—the time of Pharaoh Osorkon I…. But nothing prevents us from attributing the inscriptions of Elibaal and his son Shipiṭbaal to a later period [i.e., later in the ninth century].

Mazar did not, however, explain why he was of this opinion.

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Yet the 'traditional' absolute chronology of the alphabet at the turn of the second millennium BCE has hinged since 1947 on the conviction that Abibaal and Elibaal must have been contemporaries of Sheshonq I and Osorkon I respectively, i.e., on picking out the highest possible dates for the two Byblian kings and presenting these dates as the sole acceptable ones. Indeed, until Wallenfels' 1983 study, Mazar's 1946 view was practically forgotten as subsequent scholarship unanimously embraced Albright's unsupported ruling issued a year later (1947: 153): "It now became clear that the Phoenician [i.e., Abibaal's] text must have been incised on the statue during the lifetime of Shishak, or immediately after the latter's death.... This statue [of Osorkon I] bore an inscription of Elibaal of Byblus,...obviously almost contemporary with the statue itself." Eventually Mazar himself (1964: note 29), discarding his earlier observation, adopted this view: "It appears that Yehimelek was the father of Abibaal (the contemporary of Shishak I) and of Elibaal (the contemporary of Osorkon I), and hence Yehimelek must be placed in the time of Solomon."

The chronological discussion best begins by re-admitting that Abibaal and Elibaal were at the very earliest contemporaries of Sheshonq I and Osorkon I respectively. (These dates could be accepted if supported by additional data, but the available evidence leads rather to a lowering—see below.) In the same vein the claim that Abibaal, because his inscription is incised on the statue of the earlier pharaoh, preceded Elibaal, is best dropped. A background for the arrival of these two statues in Byblos is proposed in Sass 2002a: 252–253; according to one possibility it may have been Zakarbaal, mentioned in *Wenamun*, who originally received the earlier of the statues.

So much for the top end of Abibaal and Elibaal's time range. The bottom end of the entire Byblos sequence, probably *ca.* 750 BCE, is addressed in Sections 1.3.2 and 1.3.4 (pp. 22–34, 45–50).

1.3.1.2 Ahiram

I proceed now to integrate into the picture the only other archaic Byblos stone inscription that has a means of dating other than the palaeographic—that on the Ahiram sarcophagus. The 'traditional' *ca.* 1000 BCE dating of the sarcophagus inscription rests primarily on a combination of the following:

1. The Yahimilk–Elibaal–Šipiṭbaal lineage, recorded in Šipiṭbaal's inscription.[11]
2. The presumed contemporaneity of Abibaal with Sheshonq I and of Elibaal with Osorkon I.
3. The ensuing presumed sibling relationship of Abibaal and Elibaal.

---

[11] Whether, as is widely held, Yahimilk and Elibaal in Šipiṭbaal's inscription are the same kings as the authors of *KAI* 4 and *KAI* 6 or just namesakes is impossible to establish (see p. 47). Elibaal in *KAI* 6 presents himself as "*byḥ*...", hence probably "so[n] of Yaḥi[milk]".

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Fig. 2. Byblos, Abibaal.

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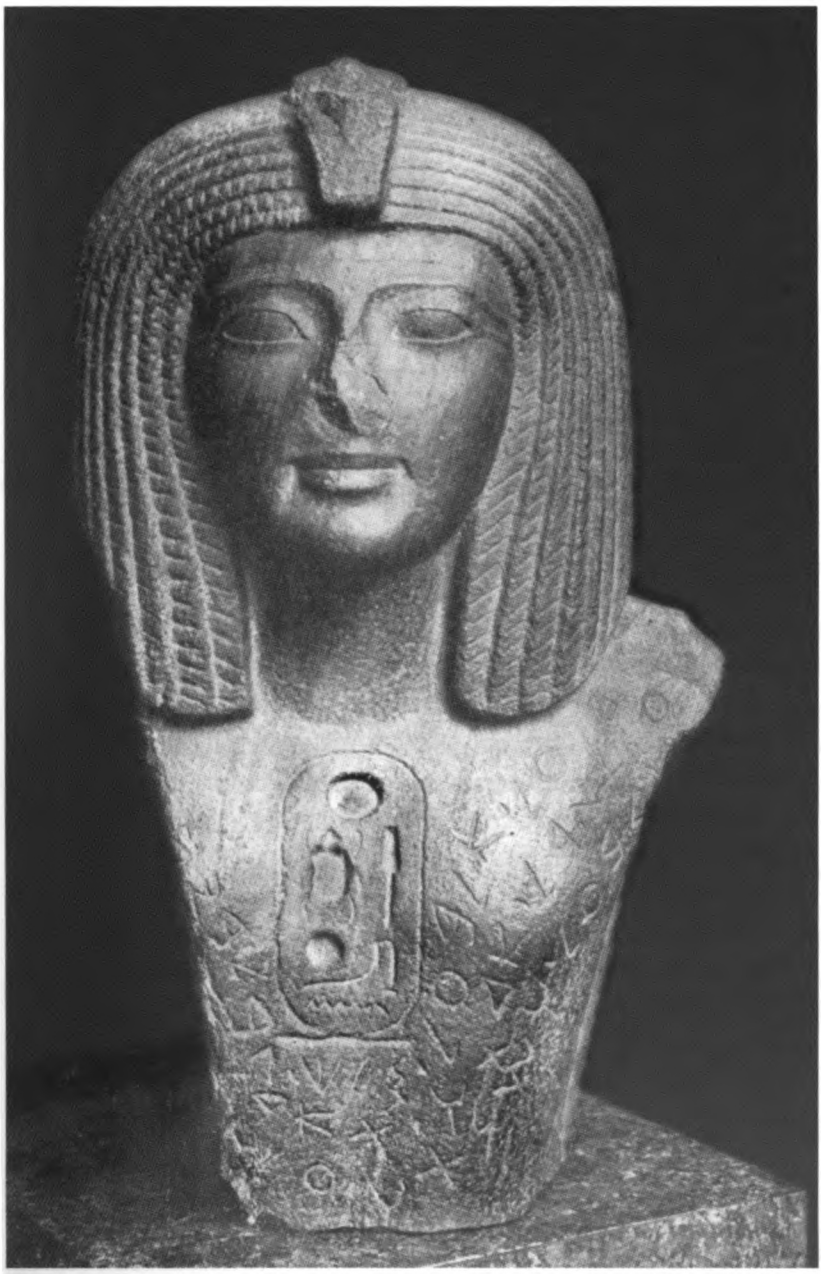

Fig. 3. Byblos, Elibaal.

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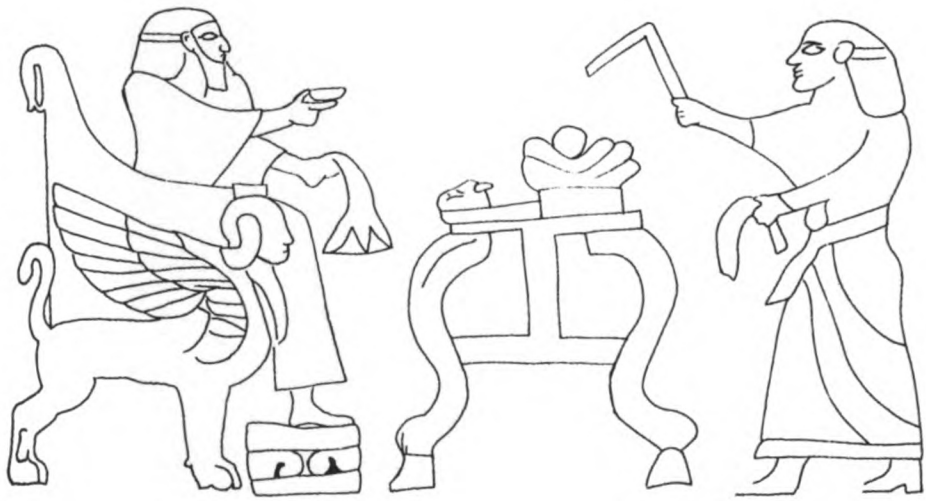

Fig. 4. Byblos, Ahiram banquet scene.

4. The ensuing presumed dating of Yahimilk prior to Abibaal and Sheshonq—in the first half of the tenth century.
5. The presumed typological seniority of the Ahiram script to that of Šipiṭbaal, and the ensuing placement of the former also before Yahimilk.
6. The presumed 1000 BCE date of certain phenomena in Ahiram's language, e.g., the 3rd m.s. pron. suffix *-h* and Gt form (*PPG*: 66 and 94 respectively).

Byblian king-lists arising from these presumptions—going back to Albright 1947—complete with regnal dates that the uninitiated may take as historical, abound in the literature.[12]

Regarding my objections to points 2–4 and the implications of the objections for absolute chronology, see above. My disagreement with point 5 is expressed in Sections 1.3.2 and 1.3.4 (pp. 22–34, 45–50). The linguistic evidence (point 6), invoked from time to time as supporting a 1000 BCE date, is in fact neutral: Whether the said phenomena are indeed archaic, or archaizing, is impossible to decide. But even if they are of chronological significance, this would at most point to the relative seniority,

[12] E.g., *KAI* II: p. 9:

Ahiram	1000 BCE
Ittobaal	975
Yahimilk	950
Abibaal	930
Elibaal	900
Šipiṭbaal	880

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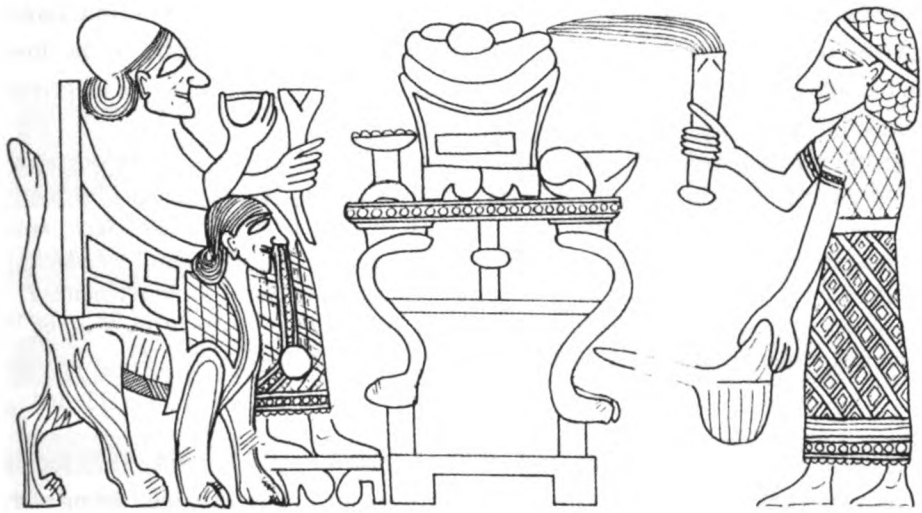

Fig. 5. Nimrud, banquet scene on pyxis from Well AJ.

or greater archaism, of Ahiram among the old Byblos inscriptions (*PPG*: 3), not to an absolute date *ca.* 1000 BCE.[13]

The Ahiram sarcophagus reliefs should play a key role in dating the inscription. Judging from the way it is cut, the inscription must be an addition to the already decorated sarcophagus.[14] The answer to the question of whether the inscription was made shortly after the reliefs were sculpted or centuries later depends on dating the reliefs. This question has been debated for years. The main reason for such uncertainty, I may add, is the Late Bronze Age roots of Levantine Iron II art: More often than not unambiguous criteria distinguish figured oeuvres from the two epochs; yet occasionally, as in the present case, the differentiation is not as clear-cut as one would wish. All the same, as I have attempted to demonstrate below, Iron II innovations in the sarcophagus reliefs rule out a Late Bronze creation.

The view expressed by Porada in 1973, namely that the reliefs belong to the orbit of Levantine Iron II art rather than Late Bronze Age art, has by now won wide

[13] Those who dated the said phenomena *ca.* 1000 BCE did so on account of the 'traditional' perception of Ahiram's age. Thus any shift in the dating of Ahiram and the other Byblos inscriptions will obviously affect the chronology of the linguistic picture. My thanks to Françoise Briquel-Chatonnet for drawing my attention to this question.

[14] E.g., Sader 1998. The manner in which the inscription commences on the raised band that frames the top of the relief on one of the narrow edges of the receptacle, continuing on the broad edge of the lid, suffices to show that it was added to the otherwise finished sarcophagus. Furthermore, the fact that it does not simply continue on the raised band that frames the principal long side of the receptacle (the one with the banquet scene) may indicate that the breaks at the right end of that band were already there at the time the inscription was cut.

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(though not unanimous) acceptance. All of Porada's parallels point to the ninth–seventh centuries, with which I agree. If, despite this, Porada ultimately came down on the side of 1000 BCE for Ahiram, it was because she acquiesced to the judgement passed on the inscription by leading palaeographers:

> Many of the features of the sarcophagus classified in this essay as belonging to the early first millennium B.C. on the basis of Assyrian monuments [of the ninth–seventh centuries], may have actually originated in Phoenicia. *Such an assumption presupposes a flourishing art in that region about 1000 B.C., of which the sarcophagus of Ahiram would be the only survival that can be recognized on the basis of its inscription, dated about 1000 B.C. by most of the leading paleographers* (italics mine—*B. S.*).
>
> Porada 1973: 364

Circularity, moreover, did not end here; palaeographers writing after 1973 took to presenting Porada's paper as an art-history corroboration of their own high dating of the inscription: "It seems, as E. Porada has shown, that stylistically nothing stands in the way of a dating [of the Ahiram sarcophagus] to the tenth century" (Röllig 1982a: 372).

Focusing on the banquet scene (see pp. 75–77), it is possible to date Ahiram with reasonable certainty: Express Iron II innovations in the relief, such as the characteristic angled fly-whisk and addorsed footstool 'brackets', are matched on an ivory pyxis from Well AJ at Nimrud. These and other shared details place the sarcophagus reliefs—hence the inscription—in the ninth century at the earliest. Taking the above-mentioned Late Bronze roots of Ahiram's Iron II imagery as evidence of a thirteenth-century origin would be like dating an archaeological assemblage by an obvious heirloom.

1.3.2 CHRONOLOGY OF THE ARCHAIC BYBLOS INSCRIPTIONS 2: PALAEOGRAPHIC CRITERIA

1.3.2.1 Introduction

If their highest possible dating is provided by the regnal years of Sheshonq I and Osorkon I (pp. 16–17), it is the script that could, at least in theory, determine the lower limit for the range of the Byblos inscriptions. Palaeographers agree that the Dan and Mesha steles, as well as other historically-dated inscriptions of the second half of the ninth century (see pp. 83–84), are written in a more developed script. On this basis the Byblos inscriptions may date anywhere between the second half of the tenth and the second half of the ninth century BCE (see note 9 on my hesitation about Kitchen's exact dates for the early 22nd dynasty). Could this range be reduced? Conversely, could the Byblos inscriptions be younger still? These palaeographical issues are explored on pp. 28–34, with the tables below supplying the data.

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TABLE 1. ARCHAIZING, NON-ARCHAIZING AND 'ECCENTRIC' LETTERS IN THE BYBLOS INSCRIPTIONS[15]

Letter	*1. Archaizing or indistinct*	*2. Inter-mediate*	*3. Non-archaizing*	*4. 'Eccentric'*	*5. Not present*
alep	Yah Ab El Šip			Ah (Bor)	
bet	Ah Yah Ab El			Šip?	
gimel	Ah Yah Ab El Šip				
dalet	Ah Yah El Šip				Ab
he	Ah	Ah Yah	Ah		Ab El Šip
waw	Ah? Yah El (Bor)		Šip	Šip	Ab
zayin	Ah Yah El Šip		Ah graffito?		Ab
ḥet	El Ah Yah Šip				Ab
ṭet	Ah Šip				Yah Ab El
yod	Ah Yah El Šip				Ab
kap	Ah Yah Ab El Šip				
lamed	Ah Yah Ab El Šip				
mem	Ah Yah El	Ah Yah Ab	Šip		
nun	El (Bor)	Ah Yah	Šip		Ab
samek	Ah				Yah Ab El Šip
ʿayin	Ah Yah Ab El Šip				
pe	Ah Yah El Šip				Ab
ṣade	Yah Ab				Ah El Šip
qop	Šip?		Yah?	Šip?	Ah Ab El
resh	Ah Yah Ab El Šip				
shin	Ah Yah El Šip				Ab
taw	Ah Yah El Šip		(Bor)		Ab

[15] The data in Table 1 is based on the collation of three of the originals (Elibaal at the Louvre, Ahiram and Yahimilk on exhibit in Paris in 1999) and of the remaining two inscriptions from photographs. The kings are listed in the 'traditional' order: Ahiram, Yahimilk, Abibaal, Elibaal, Šipiṭbaal. Also included (in parentheses) are the *alep*, *waw*, *nun* and *taw* of the Bordreuil champlevé inscription. See note 10 for all these inscriptions.

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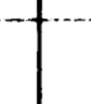
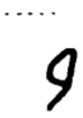
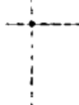
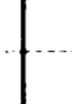
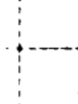
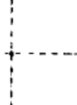
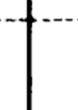
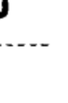
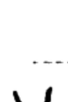
TABLE 2. DIAGNOSTIC BYBLOS LETTERS AND COMPARISONS

	Ger-baal arrow-head	*Ruwei-seh arrow-head*	*Kefar Veradim bowl*	*Tekke bowl*	*Eli-baal*	*Byblos spatula*	*Yahi-milk*	*Ahi-ram*	*Abi-baal*
alep									
bet									
gimel									
dalet									
he									
waw									
zayin									
ḥet									
ṭet									
yod									
kap									
lamed									
mem									
nun									
samek									
ʿayin									
pe									
ṣade									
qop									
resh									
shin									
taw									

◤ 'Eccentric' ◢ Non-archaizing

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Šipiṭ-baal	*Abdo sherd*	*Bord-reuil inscrip-tion*	*Fekh-eryeh*	*Kula-muwa ortho-stat*	*Kula-muwa gold ferrule*	*Dan*	*Hazael bronzes*	*Hazael ivories*	*Mesha*
K	K	K	K	K		K	K	K K	K
4	4	9	9	9		9	9	9	9
^		)	11	^		^			1
◁	4	◁	◁	◁		◁◁	4		◁
	≡		≡	≡	≡	≡	≡≡		≡
Y		Y	Y	Y	Y	Y			Y
?			I	I	I	I	I	I	I
日			日	日	?	月	日	月	月
⊖			⊗⊕						?
7	7		7	77	2	7	7		7
⋎⋎	?		⋎	⋎	⋎⋎	⋎			⋎
∠	∠	∠	∠	∠		∠	∠	∠	∠
⋋⋋			⋋	⋋	⋋	⋋	⋋	⋋	⋋
⋌		⋌	⋌⋌	⋌	⋌⋌	⋌	⋌	⋌	⋌
			三	手手	手	手手			手
○	○	○	⊙	○		○	○○		○
⋗			⋗	⋗		⋗			⋗
			⋚	⋚		⋚			⋚
φ			φ	φφ	φ	φ	φ		φ
ϥ			ϥ	ϥ		ϥ	ϥ	ϥ	ϥ
ww			w	w		w	w	w	ww
+		⋌	X	X		⋌	⋌	⋌	X

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Fig. 6. Byblos, Ahiram sarcophagus inscription (continued on next page).

Let me address first the approach which I regard as applicable to the palaeography of monumental inscriptions. Whereas in most texts written in ink the letters may reasonably be assumed to display the hand of their own time, letters engraved on hard surfaces could undergo 'monumentalization',[16] or be given archaizing shapes for a distinguished appearance (compare *WSS*: 18 for a similar phenomenon in inscribed seals). If executed properly such an inscription could be a palaeographer's lost cause.

Luckily, as demonstrated by the Fekheryeh letters (see pp. 40–43), archaism in a monumental inscription may be unmasked by two kinds of scribal slips—inadvertent later shapes and 'eccentric' shapes. The longer the inscription, the greater the likelihood of discovering such slips.

I have adopted the term 'eccentric' from Abou Assaf, Bordreuil and Millard 1982: 96. But the Fekheryeh editors had in mind a local variant diverging from the Aramaic norm,[17] with which I agree only in part. The statue script is certainly local, but it was not the everyday Gozan ductus;[18] the latter, I am sure, was the same as it was

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[16] By which is meant the occasional 're-verticalization' of letters that in the cursive became inclined top left.

[17] "[T]he Aramaic alphabet of the Tell Fekherye inscription is an 'eccentric' script. Notably the *waw*..., *lamed*..., *mem*..., *nun*..., and *ṣade*...set this script apart from all other known epigraphic texts." See also Millard and Bordreuil 1982: 140: "[W]e are faced with a local derivative of the Phoenician alphabet, perhaps adopted as early as 1000 B.C., and which continued in use and produced its unique characteristics." Amadasi Guzzo too (1991: 305) took these forms to "have undergone developments stemming from a local use."

[18] Crude but clearly following the mainstream, the Halaf 'altar' (see note 47) may document this ductus.

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everywhere else in the Aramaean world.[19] I rather regard the 'eccentric' letters (here and elsewhere) as another testimony for archaization—the scribes' hesitation about this or that obsolete letter shape, and their side-stepping of the problem by inventing old-looking forms.[20] Moreover, individual letters may display mixed characteristics, archaizing and eccentric at the same time, and even advanced and eccentric (see note 25). 'Eccentric' letters appear occasionally in monumental inscriptions; if (or when) they then resurface in non-monumental inscriptions as well I cannot tell (see pp. 31–32 on the Šipiṭbaal *bet*).

The script of a monumental inscription displaying a mix of mostly archaic forms with a few advanced and 'eccentric' forms should thus be understood as artificial, while the former shapes should be understood as archaizing. 'Eccentric' letters on their own indicate only that the script as a whole is made up and must be younger than appears at first sight. Advanced shapes, for their part, reflect the hand of the scribe's own day, the actual time of writing. This is clear in inscriptions whose dates are well established historically so that no controversy ever existed, for instance in the Barrakab inscriptions (Tropper 1993: 168).[21] I apply the same understanding to the Byblos inscriptions.

Turning again to Tables 1 and 2, what can be gleaned from subdividing the

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[19] In this I follow Naveh (1987: 104) against Kaufman. The difficulties Naveh had substantiating his viewpoint (quoted in note 44 herein) disappear with the corrected chronology.

[20] 'Eccentric' letters recur in the Hebrew script of the Persian period and afterwards (Peilstöcker and Sass 2001: 205–206).

[21] The Barrakab inscriptions combine an archaizing *kap* and *mem* with an advanced *zayin* and *qop*. Cf. Naveh 1987: 108: "True, lapidary and formal writings tend to preserve older forms. However, there are generally slips of the pen that disclose the real date of writing." Obviously, well-dated monumental inscriptions also exist whose script adheres to the cursive of their own day.

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Fig. 7. Byblos, Ahiram grafitto.

Fig. 8. Byblos, Yahimilk.

Byblos letters into archaizing, non-archaizing and 'eccentric', and from a comparison of the letters in the Byblos inscriptions among themselves and with other inscriptions? The column numbers that follow refer to Table 1.

1.3.2.2 Archaizing and non-archaizing features in the script of the Byblos inscriptions

Column 1 in Table 1 lists the archaizing Byblos letter forms as well as those forms that did not change in the transition from late Proto-Canaanite to Phoenician or during the early stages of the latter. This column includes most of the letters—the script as a whole is indeed both rather uniform and only slightly more advanced than

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Fig. 9. Byblos, Šipiṭbaal.

late Proto-Canaanite, where even the pupil-less *ʿayin* was already common (cf. note 51). Also when they differ from one inscription to another—or in some cases within the same inscription—most individual Byblos letters compared with others do not display variants of chronological value.

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Fig. 10. Byblos, Bordreuil champlevé inscription.

Columns 2, 3: In only five to seven letters can more advanced traits, cursive in origin, be discerned: *he*, *waw*, *zayin*(?), *mem*, *nun* and *qop*(?), to which may be added the *taw* of the Bordreuil champlevé inscription.

- The seven Ahiram *he*s show a mix of the legless shape, Proto-Canaanite in origin, with the more advanced form possessing a short leg. In at least one example (*mšpṭh*) the parallel strokes are slightly oblique, likewise an advanced trait—of cursive origin, even though the letter is legless. Of the two Yahimilk *he*s only one is clear, and it resembles the latter Ahiram letter. A *he* with its three parallel strokes slanting is found on the Byblos Abdô sherd, which is habitually considered contemporary with Šipiṭbaal because of the shared peculiar *bet* (see pp. 31–32). The Šipiṭbaal inscription itself contains no *he*.
- The Elibaal and Yahimilk *waw*s are of the archaic 'crescent on pole' shape. Some of the Ahiram *waw*s display small variations on this form, of uncertain chronological significance. The two Šipiṭbaal examples are indeed the most developed in that the head is on the left of the shaft, a characteristic not otherwise documented in historically dated Phoenician–Aramaic texts before the Kulamuwa and Dan inscriptions of the second half or last third of the ninth century.[22] The oblique leg of the Šipiṭbaal *waw* could at the same time point in the 'eccentric' direction, for which see p. 32.
- In Ahiram's graffito (*KAI* 2) the *zayin* is broader and lower than in the other inscriptions. This in itself is an advanced trait, but the difference in our particular case may be too small to carry weight.
- The most archaic-looking Byblos *mem* and *nun* are found in Elibaal, the former

[22] But it should be remembered that not even a single 'post Proto-Canaanite' inscription is dated *ca.* 900–850/830 with absolute certainty (cf. p. 83).

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letter with its bottom stroke the same length as the others, the latter with its outer strokes the same length and the middle one shorter.[23] (A similarly proportioned, though angular *nun* is found in the Bordreuil champlevé inscription, p. 33.) One or two of the Ahiram and Yahimilk *mem*s may also be of the same kind, whereas in the others, and nearly in all *nun*s of these inscriptions, the bottom stroke is slightly longer than the others—the beginning of a leg. Šipiṭbaal's *mem*s (to a certain extent also the *nun*s) display the longest, most advanced legs, and one of the *mem*s (first *mlk*) also has a slightly oblique rather than vertical head, another developed feature. If the relative dating of the Byblos inscriptions were founded on a strictly linear development of the letter shapes, Šipiṭbaal's *mem*s would agree with this king's position as last in the documented succession of three reigns, and latest among the five discussed. That these shapes most probably are not enough for such a conclusion see pp. 46–48.

- In the Yahimilk *qop*s the shaft bisects the circular head, as it generally did in the ninth century,[24] and perhaps before. In the Šipiṭbaal *qop* the shaft begins underneath the head, a seemingly earlier trait known from the ʿIzbet Ṣarṭah ostracon (but see note 49). Yet the head is not circular as one would expect but rather 'noose shaped', a unique, probably 'eccentric' form (see p. 32).
- On the advanced *taw* of the Bordreuil champlevé inscription see pp. 32–33.

1.3.2.3 'Eccentric' features in the script of the Byblos inscriptions

Column 4 in Table 1 lists what I interpret as the 'eccentric' forms, two to four in number—*alep*, *bet*(?), *waw* and *qop*(?).[25]

- The unusual Ahiram *alep* (it recurs in the Bordreuil champlevé inscription, below) was long understood as a local by-form of *ca.* 1000 BCE.[26] Yet its two curved 'arms', and the manner in which they join the vertical, diverge enough from the line of development of this letter to justify the inclusion of the form in the present category.
- The Šipiṭbaal *bet.* The same shape is present in one or two non-monumental inscriptions—on the Abdô sherd from Byblos (*KAI* 8) and possibly in a

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[23] Forming part of a curving line of script, the intended stance of this *nun* is uncertain.

[24] Sass 1988: 131, still according to the 'traditional' chronology.

[25] The Šipiṭbaal *waw* is an 'advanced–eccentric' hybrid—with an invented trait added to a non-archaizing shape. The Šipiṭbaal *qop* possibly is an 'archaizing–eccentric' combination—with an invented trait imposed on an already archaic-looking shape.

[26] "It is now certain that the peculiar quasi-cursive *aleph* of Ahiram is a local graphic peculiarity, which did not last long, since it is absent from both earlier and later inscriptions" (Albright 1947: note 14).

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Phoenician(?) graffito from Stratum VIII at Hazor (pp. 85–86). The traits in the Abdô script, which are more advanced than in the Byblos stone inscriptions,[27] and the date of Hazor Stratum VIII in the last third of the ninth century, would support placing Šipiṭbaal at about the same time. On the other hand, as 'eccentric' shapes are less expected in non-monumental inscriptions, such an identification of the Šipiṭbaal *bet* would have carried more weight had the same form not also appeared on the Abdô sherd.

- The Šipiṭbaal *waw* is in the shape current from the ninth century onwards (see p. 30) except for the strong inclination in the wrong direction of its leg, a trait which may mark this letter as 'eccentric' at the same time. Practically everywhere else the leg is vertical, or slightly leaning top left.
- Nor does Šipiṭbaal's 'noose shaped' *qop* follow the evolution of the Phoenician form. The divergence from the archaic 'circle on shaft' letter (see p. 31) may be an added 'eccentric' trait.

Several features in the Byblos letters are thus best understood as 'eccentric'—invented substitutes for forgotten archaic forms, or an enhancement of current forms, designed to make them look archaic. That my interpretation of these letters may apply is borne out by similarly outlandish forms in the Fekheryeh inscription, forms which likewise cannot be fitted into the line of development of the alphabet (see pp. 40–43). In the case of the Byblos inscriptions the recognition of 'eccentric' letters may be particularly relevant for Ahiram: It removes what was hitherto regarded as a by-form—the *alep*—which has somehow become salient in the effort to date the text *ca.* 1000 BCE. The 'eccentricity' of this shape of *alep* is especially pronounced in the Bordreuil champlevé inscription.

1.3.2.4 The Bordreuil champlevé inscription, and an interim balance of the Byblos archaism

How does the champlevé inscription (Bordreuil 1977), of unknown but presumed Byblian provenance, fit into this picture? In fact it displays an admixture of palaeographical traits even more revealing than in Šipiṭbaal: Its developed Phoenician–Aramaic 'scalene' *taw* is not found in any other Byblos inscription;[28] the earliest historically-dated attestation of this shape is in Kulamuwa and Dan (see p. 83), *ca.* the 830s (but see note 22). Cross (1995: 398) observed that this type

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[27] *Dalet* with a leg, *he* with its three parallel strokes slanting. These forms begin in the ninth century and continue into the eighth.

[28] The *taw*s of the five royal Byblian inscriptions are all of the archaic, isosceles type: Yahimilk and probably Elibaal are x-shaped, Ahiram and Šipiṭbaal +-shaped. In Abibaal no *taw* is preserved.

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of *taw* in Phoenician was an innovation of the late ninth century: "Both Hebrew and Phoenician *taw* retain an X-form, in Hebrew for centuries, in Phoenician through the ninth century." The Bordreuil inscription thus appears to be of the second half of the ninth century at the earliest. Combining as it does (among other interesting forms) a very archaic-looking *waw* and *nun*, Ahiram-type 'eccentric' *alep* and the said advanced *taw*, the Bordreuil inscription belies the 'traditional' chronology and substantiates my dating of Ahiram to the ninth or eighth century and general lowering of the Byblos inscriptions in a manner that is almost too convincing. That this could be overlooked, and the ninth–eighth-century-type Bordreuil *taw* revised upward instead by 200 years, emphasizes how the 1000 BCE dating of Ahiram has risen to the status of an unassailable datum.[29] (Consequently 'traditionalists' anxious not to lose their ground will probably now condemn the Bordreuil inscription as a forgery.) Fortunately it is unnecessary to hinge on this fragment the down-dating of all other archaic Byblos inscriptions; they contain sufficient evidence to that effect themselves.

The archaism of the Byblos script, then, was a phenomenon of at least a short duration, say an overlap of the later of the five Byblos inscriptions with texts of the second half of the ninth century like Kulamuwa and Tel Dan.[30] But are there indications that the Byblos script was put together *after* the ninth century? This cannot be decided with palaeographical tools alone; the most I can say is that the potential eighth-century forms in the Byblos inscriptions seem to be only those that had already begun in the ninth: The five Byblos inscriptions (and for that matter the Bordreuil champlevé inscription) do not contain expressly eighth-century forms such as a vertical-headed *mem* or asymmetric *qop* (cf. Table 11 on p. 144). And typical seventh-century shapes are absent, too. I return to the question in Section 1.3.4 (pp. 45–50), where the relative and absolute chronology of the Byblos kings is examined. The problem of the end, in turn, is linked with that of the duration and beginning: When was the earliest Byblos inscription written? Does archaism characterize only

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[29] Röllig, for instance (1982a: 369), because of the archaic-looking *waw* and *nun* just mentioned, was for dating the champlevé inscription even earlier than Bordreuil's tenth century, yet he said nothing on the advanced *taw*. However, in his paper on Sardinia published that same year (1982b: 127), Röllig may have referred obliquely to this *taw*: "Noteworthy are the variants of the *taw*, that also in the older Byblos inscriptions does not possess a fixed form, but oscillates between an upright shape with a longer vertical and a more leaning form with nearly equally long strokes" (see also note 33). To me the artificiality of the archaic-looking letters of the champlevé inscription is accentuated by this ninth–eighth-century *taw* in their midst.

[30] According to this reasoning the Byblos inscriptions also overlap in part the monumental Phoenician inscriptions from Cyprus, Nora and Bosa (p. 83), 'colonial' but likely to follow the pattern of the motherland. The Cypriot and Sardinian inscriptions display a writing more developed than that of Byblos and not very far removed from that of Kulamuwa. Yet I hesitate to emphasize this point, for the island inscriptions have no dating besides the palaeographical.

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the later of the five inscriptions, so that the script of the earlier ones does after all reflect the current hand of their day? Or did even the earliest Byblos inscription follow a mostly obsolete ductus, and if so, what could the source of this writing have been? The working hypothesis emerging from Section 1.3.4 (pp. 45–50) is that the Byblos inscriptions date to *ca.* 850–750 BCE whereas the majority of their letters emulate the ductus of the first half of the ninth century. In order to put these points in perspective I turn first to the inscriptions considered to immediately precede Byblos.

1.3.3 CHRONOLOGY OF THE LATEST PROTO-CANAANITE PHASE

The redating of the Byblian inscriptions evidently affects the latest Proto-Canaanite inscriptions. Most numerous among the latter are short texts incised on bronze arrowheads. Except for one, the Ruweiseh arrowhead, discovered, although out of context, in an archaeological excavation (cf. Sass 1988: 82–83), these arrowheads come from illicit digs—some perhaps, or at any rate their inscriptions, from forgers' workshops.[31] In either case they have no dating other than the palaeographical. (Nor is object typology of much help; similar arrowheads are known from late LB to early Iron II—Cross and Milik 1956: 19–21.[32]) With a script that is for the most part somewhat less developed than that of the Byblos inscriptions and hence was hitherto dated to before *ca.* 1000 BCE, the arrowheads' lower limit, according to the chronology employed in this book, would move downwards. Fortunately that limit, in the first half of the ninth century, can at the same time be established independently by archaeological context: Excavations of two ninth-century tombs, one in Crete, the other in Israel, each yielded an inscribed bronze bowl with letters that are quite similar. The archaizing, late-ninth-century Tell Fekheryeh inscription is also written in a mostly late Proto-Canaanite hand. Sections 1.3.3.1 and 1.3.3.2 deal with these texts.

1.3.3.1 Two inscribed bronze bowls and their archaeological contexts

Tekke

Tomb J at this site near Knossos contained a bronze bowl with a late Proto-Canaanite inscription. Remarks on the assemblage and on the inscription follow.

The assemblage and its dating. Pottery vessels imported from Attica date the two burials in Tomb J to the very end of the Late Proto-Geometric period. In the preliminary report the Attic vessels were assigned to the earlier, the bronze bowl to the later of the two burials (reference in Sass 1988: 89). The final publication toned

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31 This doubt may pertain much less to exemplars published in the 1950s and 60s, before the flood that began in the 70s.

32 Nearly half a century after this pioneering work, a new typological study would certainly be rewarding.

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Fig. 11. Tekke bowl.

this down: "We cannot know whether the imports go with the earlier, or with the later, or with both burials.... [T]heir excellent condition...makes it likely that some of them, at least, were offered with the later burial" (Coldstream and Catling 1996: 410).

The end of PG is habitually placed *ca.* 900 BCE. Proto-Geometric pottery discovered in the last decade in late Iron I contexts at Tel Dor, Tel Rehov and Tel Hadar in Israel points to a similar dating (see p. 153). Dependent on the Levant as Greek chronology of the period is, Gilboa and Sharon's comment (2001: 1347) on even lower dates is of relevance: "[T]he transition from Middle to Late Proto-Geometric in Euboea...according to the absolute [radiometric] chronology at Dor,...cannot antedate the turn of the 10th century, at the very earliest." (Customarily, the MPG–LPG transition in Euboea as well as in Attica is dated *ca.* 950 BCE—cf. Lemos 2002: 26.)

In a few years when, as I hope, the accumulation of ^{14}C dates will have stabilized the Palestinian chronology of the period, this will also have bearing on the date suggested herein for the Tekke bowl and its inscription (see further p. 66). Luckily the bowl was since joined by another, one that likewise points to the time about or after 900 BCE (below).

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The inscription. Inspired by the late Proto-Canaanite type of the letters, adherents of the 'traditional' epigraphic chronology sought to date the bowl some 200 years earlier than its archaeological context, claiming at one point that what had to be revised drastically upwards was Late Proto-Geometric chronology (references in Sass 1988: 90). To me this seemed impossible (1988: 88–91) and it still does. For the inscription, I much prefer a date "towards 900" as was Sznycer's suggestion in the *editio princeps* (1979), if hesitatingly (cf. Sass 1988: 90), followed resolutely by Röllig (e.g., 1995: 204),[33] and underpinned by the just-mentioned dating of the assemblage.

Kefar Veradim

This site is located in the Upper Galilee, 17 km northeast of Acre. The early Iron II burial cave No. 3, excavated in 1995 (Alexandre 2002a), contained a bronze bowl with a late Proto-Canaanite inscription (Alexandre 2002b). The inscription was revealed in 1997, when the bowl was cleaned (Alexandre 2002b: 65, and pers. comm.). My remarks refer to the tomb assemblage, the bowl, and the inscription.[34]

The Assemblage and its dating. Alexandre (2002a: 61–62) equated the pottery from the tomb to the assemblage of the main phase (II) of nearby Horvat Rosh Zayit, the latter compared by the excavators to Megiddo VA–IVB and Hazor X–IX. On the ninth-century date of this phase at Rosh Zayit cf. Finkelstein 2002: 124–126.[35]

The bowl. The following addresses the absolute dating and possible origin of the Kefar Veradim bowl, as well as the background for the manufacture of this particular type.

1. *Date*. Coming from a clear early Iron II context, the bowl is the earliest of its kind found in the southern Levant. Well-dated comparisons from Assyria for the particular gadrooned type begin in the ninth century. Alexandre (2002b: 72) has carefully and judiciously assessed this situation, but by assigning as she did the tomb assemblage to the tenth century, she sought to date the bowl accordingly. Taking as I do the absolute date of the Kefar Veradim assemblage (and of early Iron II at large) to be in the ninth century, my simplest solution concerning the bowl is to regard it as contemporary with the assemblage: The

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[33] Even while affirming the *ca.* 900 BCE dating for the Tekke bowl, both authors maintained their faith in the 1000 BCE date of the palaeographically more advanced Ahiram inscription (Sznycer 1994: 171; Röllig 1995: 204; similarly Amadasi Guzzo 1991: 305). In his turn, Wallenfels (1983: 98–99) underestimated the support the Tekke bowl and its archaeological context lend to his down-dating of the Byblos inscriptions.

[34] Yardenna Alexandre kindly read an earlier version of this section. Her comments, gratefully acknowledged, are incorporated.

[35] Gal and Alexandre (2000: 150–152) dated the said phase to the tenth century ('traditional' chronology), linking it with the Land of Cabul of the Solomon–Hiram narrative in 1 Kgs. 9: 13.

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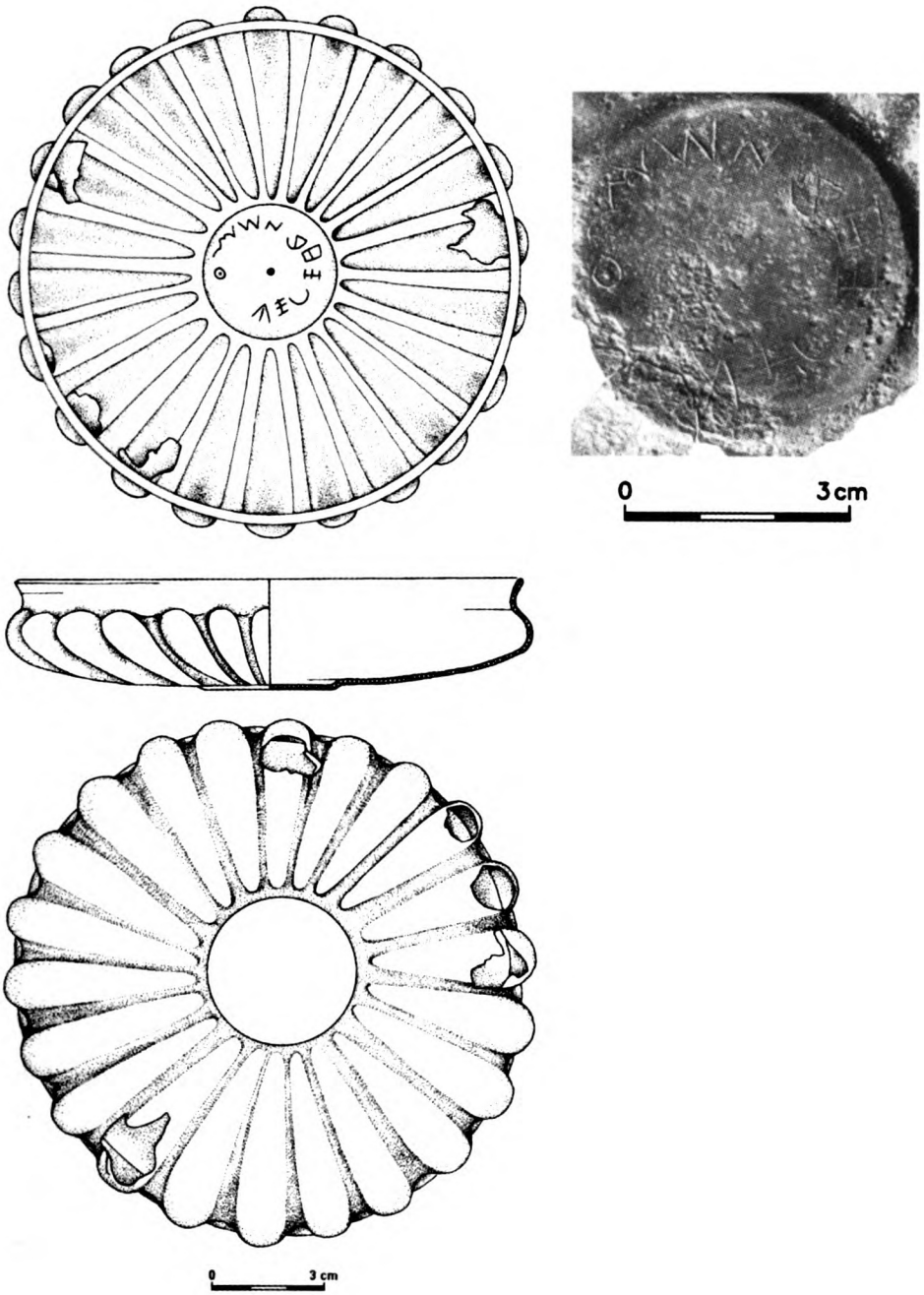

Fig. 12. Kefar Veradim bowl.

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discovery in the southern Levant of a specimen as old as its earliest Assyrian and north Syrian counterparts is already exceptional, and Alexandre was the first to underline this.[36] To raise its date by yet another century, making it the earliest-known bowl of this kind in the entire Near East, requires very strong evidence indeed, which in the present case does not, I believe, exist.[37]

2. *Origin*. The question of whether we have before us a rare pre Tiglath-pileser III Neo-Assyrian import or a local product cannot be answered with certainty.[38] Following the data presented by Alexandre, yet adjusting the absolute chronology, a Levantine manufacture of the ninth century is, however, a distinct possibility; likewise Alexandre's suggestion (2002b: 71–72) that the bowl held by Ahiram may be of a similar form.[39]
3. *Background*. Gadrooned Ramesside bowls almost certainly are the ultimate parentage of the Iron II bowls (Alexandre 2002b: 72; see further Addenda, p. 183 herein). Yet as far as I can tell, the latter bowls begin with the ninth-century revival in Assyria and the West Semitic realm. Like most Iron II prestige items and art, they possess Late Bronze roots, but do not have, and in the cultural and political configuration of the time can hardly be expected to have, eleventh–tenth-century Iron I antecedents.[40]

The inscription. I disagree with Alexandre (2002b: 68) that the Kefar Veradim inscription is some 50 years older than the one on the Tekke bowl: At first sight the letters may give a more archaic impression, but closer scrutiny reveals this to be derived mainly from the unrefined 'hand'.

I wish to bring into the discussion a Rosh Zayit sherd with a Phoenician ink inscription that comes from the above-mentioned early Iron II context contemporary with Kefar Veradim (Gal and Alexandre 2000: 133–134;[41] not

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36 2002b: 71: "[U]ntil the discovery of the Kefar Veradim bowl there was little evidence for such bowls in the Levant prior to the late Iron Age (Assyrian period) or even the Persian period."

37 Comparing the absolute dates of early Iron II between the northern and southern Levant while employing the 'traditional' chronology for the latter is bound to create confusion—cf. p. 66.

38 My sincere thanks to Baruch Brandl and Tallay Ornan for discussions on the possible origin of the bowl.

39 Alexandre followed the 'traditional' *ca.* 1000 BCE dating for Ahiram. On the post-tenth-century date see pp. 20–22, 75–82.

40 On the general phenomenon see pp. 22, 77. On the particular type of bowl see Alexandre 2002b: 70: "There was, however, a complete dearth of evidence from the eleventh–tenth centuries BCE [tenth–ninth according to the corrected chronology, dearth still valid for eleventh–tenth, *B.S.*] until the discovery of the Kefar Veradim bowl. In the ninth century BCE fluted bowls begin to appear in Assyria and [northern] Syria."

41 In *HAE* I (pp. 37–38) Renz waivered between a Hebrew and a Phoenician attribution, whereas Lemaire (1997: 162), whom I follow here, was firmly for the latter.

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mentioned in Alexandre 2002b). The sherd's *mem* with a leg and an oblique head and the long-legged *nun* are far more advanced than the letters of the similarly dated bowl. To me this seems to corroborate the suggested concurrence in the early ninth century of the late Proto-Canaanite and early Phoenician scripts (see p. 52).

Conclusions. If the 'traditional' or high chronology for West Semitic palaeography and Palestinian archaeology is applied, an absurd situation ensues: the Kefar Veradim inscription would be 200 years older than the bowl it is written on, with the tomb assemblage sandwiched between the two. In other words, the inscription would date to the eleventh century, the tomb assemblage to the tenth, and the bowl to the ninth.[42] With the chronology introduced in this book all three—the tomb, the bowl, and the inscription—coincide in the ninth century.[43]

The Tekke and Kefar Veradim bowls—Conclusions

Published in 1979 and 2002 respectively, the two bowls were unearthed in intact burial caves. These are the first objects with West Semitic linear alphabetic inscriptions of the period *ca.* 1150–850 BCE to be discovered in secure archaeological contexts, and as such their value for establishing at long last a reliable dating cannot be overestimated. (Similarly Alexandre 2002b: 69: "[O]n the basis of its clear archaeological context...[t]he Kefar Veradim inscription may...provide a much sought-after chronological peg for early first millennium BCE inscriptions.") The 'traditional' palaeographical dating for the late Proto-Canaanite inscriptions on both bowls is the eleventh century, whereas the archaeological contexts are in the ninth. As long as only the Tekke bowl was known, supporters of the high chronology could argue that it is an heirloom in a context some 200 years later. The cumulative evidence of the Fekheryeh archaism, Ahiram reliefs and now Kefar Veradim bowl (both archaeological context and bowl typology for the latter) puts this argument out of court.

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[42] Also Alexandre (2002b: 68–69) found an 11th-century date for the inscription—'traditional' epigraphy chronology—too high: "Naveh...considers that the evolution of the individual letters...points to...the first half of the eleventh century.... The exceptional archaeological context of the...bowl aids us in determining a date for the find, independent of the palaeographical analysis. The bronze bowl was placed in the tomb together with a collection of Iron IIA–B domestic ware and fine Cypro-Phoenician Black-on-Red pottery, a homogeneous repertoire dated to the tenth century. It is theoretically possible to claim that the inscribed bowl was handed down as an heirloom over a period of some 100 years before it was laid to rest with its last owner.... However, given the fact that the eleventh century date for the inscription is not unequivocal, together with the extreme dearth of dated inscriptions, the most plausible interpretation is that the inscribed bowl should be dated to the tenth century...."

[43] The Kefar Veradim bowl may now become a catalyst for the addition of forged inscriptions to genuine Iron Age metal bowls, in a manner similar to that which probably occurred with the arrowheads.

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1.3.3.2 The Tell Fekheryeh script

Immediately after its discovery in 1979, when only the Aramaic inscription had been studied, the Tell Fekheryeh statue was dated to *ca.* 1100 BCE, in line with the 'traditional' palaeography chronology (references in Kaufman 1982: 139; Bordreuil 1993: 263). Yet when the orthography and language of the Aramaic text, the various aspects of the cuneiform text and the style of the statue were evaluated, all in conjunction with historical considerations, that dating turned out to be more than a quarter millennium too high (Abou Assaf, Bordreuil and Millard 1982). That the script is archaizing rather than archaic has since been acknowledged by all; yet I regard the archaization as one that harks back several decades, not 250 years and more.

Moreover, the age of Fekheryeh was evident from the beginning, based exclusively on West Semitic palaeography; a sole 'slip of the pen', to use Naveh's term (see note 21), suffices: The advanced *he*, not only legged but slanting top left in the cursive manner (see Table 2, p. 25), stands out amongst the mostly archaizing, vertical letters, and it alone is enough to make the unmistakable ninth-century date of the inscription plain for all to see. This was pointed out succinctly by Cross (1995: 403), following Abou Assaf, Bordreuil and Millard 1982: 89:

> [T]he letter *he* has some advanced traits. In many exemplars we find a rotation of the letter counterclockwise from the vertical, the horizontals inclining downward to the left. This...shift of stance is observed first of all in ninth-century Phoenician, Hebrew, and Aramaic.... One should not overlook the fact that the crude *ḥet* of the Gozan text [i.e., Halaf 'altar'—see note 47] shows this shift of stance....[44]

To the *he* I may add the *nun*s, about a fifth of which display a short leg, likewise an advanced trait.

The letters labelled 'eccentric' indicate as well that at the time the Fekheryeh text was carved, the type of script represented by most of its letters was no longer in everyday use. Slightly at variance with the Fekheryeh editors (see p. 26 with note 17), as well as with my earlier observations (e.g., Sass 1988: 123), I now see 'eccentricity' in the following:

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[44] On this *he* see also Sass 1988: 160, note 107, still according to the 'traditional' chronology. Differently Naveh 1987: 107: "[T]he slant of the *he* is not definitive evidence." *Ibid.*: 109: "[I]ts [i.e., the statue's] set of letters was copied *without a single failure* from a stele of the eleventh century [italics mine—*B.S.*]." In this Naveh took into account neither the *he* nor the five 'eccentric' shapes (for which see main text). He went on to say that "this is merely another excuse, because there is no indication for such imitations ever having existed in ancient West Semitic writing." To me the Fekheryeh script is neither an imitation of an eleventh-century hand, nor necessarily a copy from a stele (cf. pp. 50–58).

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TABLE 3. ARCHAIZING, NON-ARCHAIZING AND 'ECCENTRIC' LETTERS IN THE TELL FEKHERYEH INSCRIPTION

Letter	*1. Archaizing or indistinct*	*2. Non-archaizing*	*3. 'Eccentric'*
alep	+		
bet	+		
gimel	+		
dalet	+		
he		+	
waw			+
zayin	+		
ḥet	+		
ṭet	+		
yod	+		
kap		+	+
lamed			+
mem	+		+
nun	+	+	
samek	?	?	?
ʿayin	+		
pe	+		
ṣade			+
qop	+		
resh	+		
shin	+		
taw	+		

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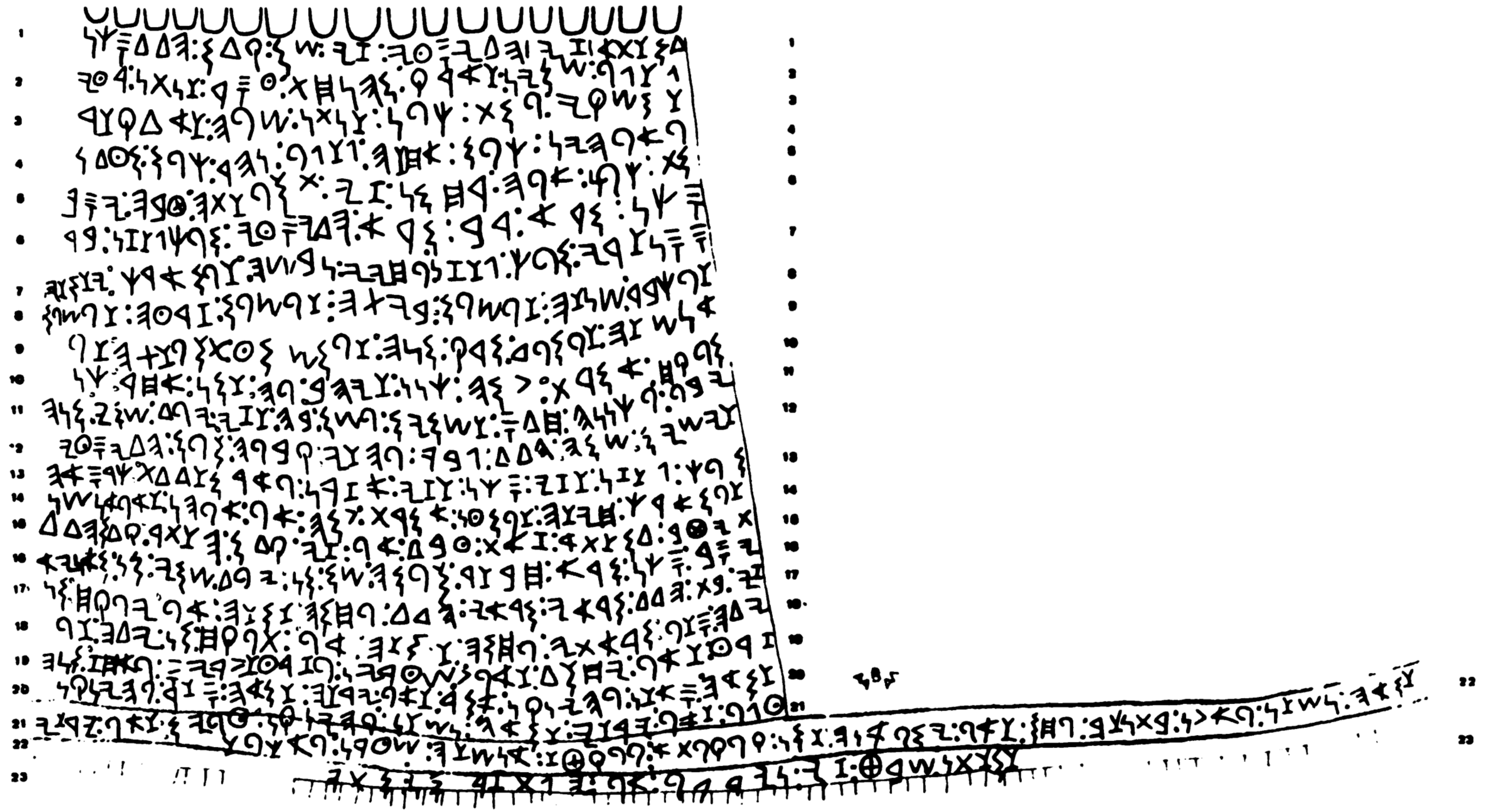

Fig. 13. Fekheryeh inscription.

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- The *waw*'s baseline.
- In that the *kap*'s leg, itself a non-archaizing trait, is a (vertical) continuation of the middle finger rather than an (oblique) continuation of the right-hand finger as in mainstream Phoenician–Aramaic *kap*s of the period.[45]
- The inversion and 'question mark' shape of the *lamed*.
- The unusual position of the *mem*'s leg.[46]
- The outlandish shape of the *ṣade*.

Whether the Fekheryeh *samek* should be classified as archaizing, non-archaizing or 'eccentric' remains for me an open question (in this I follow Naveh 1987: 108–109).

Also the ample use of *matres lectionis* points in the direction of the ninth century (cf. further pp. 51–52). And see pp. 61–66, 93–95 on the historical setting, and on dating Hadyisʿi, the author of the inscription, in the 840s or 820s.[47]

1.3.3.3 The arrowheads and the last Proto-Canaanite phase (tenth–ninth centuries)—Conclusions

Judging by their archaeological contexts, the Kefar Veradim and Tekke inscriptions belong in the first half of the ninth century. This, rather than the eleventh century, is then also the date of the quite similar ductus imitated by Fekheryeh. The script on the bowls, in turn, is close to the letters on several of the arrowheads, the latter as a group displaying some variety, hence a life-span of some duration. It is impossible to gauge the length of this span since no anchor exists to link the beginning of the arrowheads to absolute chronology (see p. 34). As a working hypothesis I would suggest that, since they last into the ninth century, the arrowheads began at

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45 The latter phenomenon may be compared to the obliqueness of the leg of the Šipiṭbaal *waw*, both going against the development of their letters. Usually the *kap* has an oblique leg, the *waw* a vertical one (see p. 32 and note 51). On the validity of the comparison with the ʿIzbet Ṣarṭah *kap*, see note 49.

46 The 'eccentric' addition (as I see it) of a short vertical stroke to the vertical *mem* may well combine knowledge of the stance and shape of the late Proto-Canaanite letter with the notion, dating to the scribe's own time—the late ninth century—that a *mem* was incomplete without its leg.

47 Not archaizing (in case it is to be assigned to the ninth century and not later), merely crude, is the inscription on the so-called miniature altar found at Tell Halaf, across the Habur from Tell Fekheryeh. Dankwarth and Müller (1988) dated it to the first half of the ninth century and this was supported by Röllig (2000: 180–181); cf. also Röllig 2003: 427: "[T]he…'altar', with the oldest Aramaic inscription from northern Syria, is in fact a statue base." To me the said dating seems more precise than the evidence permits. All the same the K-shaped *kap*, taken together with the generally archaic ductus, could point to the ninth century at large. Yet the lack of a clear archaeological context, combined with the careless script, diminish the value of the 'altar' inscription in palaeographical comparisons.

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Fig. 14. Byblos cone A.

a certain point in the tenth.[48] But the unstratified Byblos cone A (Sass 1988: 79–80) and some of the arrowheads' legends display a mix of Proto-Canaanite and cursive features, occasionally even in one single letter (e.g., a mirror-image yet legged *mem* and a mirror-image yet slanting *ḥet* in cone A). This may alternatively confine them to the transitional period from Proto-Canaanite to the Phoenician–Aramaic (and Hebrew) scripts, *ca.* 900–850 BCE (for the transitional period see p. 52). Could this mix indicate that cursive writing began even a little earlier, during the 'New Canaan' phase of the tenth century (see p. 67)? The question must remain unanswered, as no inscriptions are known thus far from the late Iron I strata at the relevant sites—Megiddo, Tel Kinnerot, Tel Rehov, etc.

1.3.3.4 The antecedents: the penultimate Proto-Canaanite phase (twelfth–tenth centuries)

Given the above shake-up, what inscriptions remain in Iron I, *ca.* 1150–900 BCE? Evidently the Raddana handle (Sass 1988: 58–60, 152; Lederman 1999: 139–142) and ʿIzbet Ṣarṭah ostracon (Sass 1988: 65–69, 152).[49] Unstratified, but coming from

[48] My 1989 conclusions on the contemporaneity of the Levantine and Babylonian inscribed arrowheads will have to be revised.

[49] On the value of the ʿIzbet Ṣarṭah ostracon for palaeographical comparison, I follow Naveh (1978: 35): "His [the scribe's] confusion of letters and his mistakes seem to be so serious that I would not recommend the drawing of paleographic conclusions from any of the forms produced by him. We cannot know which of the letter forms are based on the contemporary scribal tradition and which are the products of either the writer's poor training or his bad memory." This supersedes my older view (1988: 66).

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a site settled only in Iron I and the beginning of Iron II, the ʿIzbet Ṣarṭah ostracon cannot be dated more closely than to the entire range of the said period, i.e., *ca.* 1150–850 BCE, unsatisfactory though this may be. Palaeography does not help in narrowing this range (see note 49). Raddana was assigned by Lederman (1999: 73–74, 140) to the very end of Iron I, i.e., the tenth century. (Higher dates in the ʿIzbet Ṣarṭah and Raddana excavation reports.) The seeming gap between this date and the older-looking letters of the Raddana inscription led Lederman (*ibid.*: 142) to venture, among other options, that the handle, which he concluded was a surface find, was brought to Raddana from another, earlier site.

In addition to these two texts, some dateless inscriptions may belong here (Sass 1988: 64–65, 69–72), nearly all from the southern Levant—see pp. 54–55 on the distribution—and possibly the earliest arrowheads from the tenth century. A small yield indeed, which, under the circumstances outlined on p. 51 should come as no surprise.

1.3.4 Chronology of the archaic Byblos inscriptions 3: the extent of the archaism

A dating later than the tenth century for all archaic royal Byblos inscriptions becomes necessary not only in the light of certain details in Ahiram's banquet scene, for which no pre-ninth-century comparisons are known (pp. 75–76), or the few ninth-century (or early-eighth-century) features in the otherwise archaizing script of the Šipiṭbaal and Bordreuil champlevé inscriptions (pp. 32–33). The probability of such a dating arises even more strongly from the ninth-century date of the Tekke and Kefar Veradim bowls (pp. 34–39), their late Proto-Canaanite script less developed than that of the Byblos inscriptions; and it fits the cultural and political situation in the Levant (see Section 1.4.2, particularly pp. 66–67). As West Semitic monumental art and inscriptions, and the Aramaic and Hebrew scripts, do not otherwise begin before the ninth century (see pp. 55–64), the lingering faith in the 'traditional' tenth-century dating of Byblos has rested on the notion that the Ahiram inscription was somehow different; that monumental art, and inscriptions in a 'national' West Semitic alphabet, could begin in Byblos 100–150 years earlier than anywhere else in the Levant (compare pp. 17–22 on Ahiram's reliefs and inscription). True enough, Phoenicia perhaps had a smoother Bronze-to-Iron-Age transition than most of the West Semitic realm, but writing there prior to the tenth century was probably not in the linear alphabet (see pp. 53–55, 66–67). Thus, we are left with a post-tenth-century date for all the five Byblos inscriptions.

But can a time-frame more specific than merely 'post tenth century' be established? In the next three sections I explore the possibility of arranging the five

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Byblos kings in a palaeographical sequence, the option of assigning absolute dates to at least the beginning and end of this sequence, and the presumed source of the script.

1.3.4.1 Relative chronology: palaeography and the sequence of the Byblos kings

Had the Byblos inscriptions spanned several decades or as much as a century of *living* script, a distinct palaeographical development would be expected. Yet a glance at Tables 1 and 2 demonstrates how most letters—e.g., the legless *dalet* and *kap*, the tall *zayin*—correspond to a uniform archaic pattern (Column 1); the lack of development underlines the artificiality of the script. (That the Byblos inscriptions, except the later-looking Šipiṭbaal, were written according to a uniform pattern was observed in the 1940s by Mazar [1946: 174 = 1986: 289] and Albright [1947: 153–154].[50]) Just a total of 5–7 non-archaizing and 2–4 'eccentric' letter forms (Columns 2–4 in Table 1) are found in all Byblos inscriptions. For comparison, Fekheryeh (see pp. 40–43) has 3 non-archaizing letters and 5 'eccentric' ones.[51]

All the same, two of the Byblos inscriptions may be quite revealing: In Šipiṭbaal there are sufficient non-archaizing features to indicate a post-tenth-century dating. Indeed, the ninth-century traits in Šipiṭbaal were recognized long ago, but most authors were for a dating at the beginning of the century because of the presumed links with Elibaal and Yahimilk, in turn presumed to belong to the tenth century. The Šipiṭbaal *bet*, together with the presumably contemporary Abdô *dalet* (p. 32 with note 27), may make further refinement of the dating of these two inscriptions possible: They are probably no earlier than the second half of the ninth century. Šipiṭbaal's *waw* and *mem* fit this dating as well. On the other hand, non-archaizing and 'eccentric' shapes seem to be absent altogether in Elibaal.

Since the Byblos script is largely archaizing, however, one must first consider whether the few non-archaizing letter shapes offer adequate evidence for arranging the inscriptions in a chronological order. Having given it a try I am rather discouraged. Nevertheless, the outcome is presented below; I thought that the reader should have the option to form an opinion about this attempt even if it did not yield the hoped-for results. The starting point is Tables 1 and 2, together with pp. 28–31:

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[50] See also Millard 1991: 102: "The Ahiram sarcophagus can fit happily earlier than those [Abibaal, Elibaal, Yahimilk and Šipiṭbaal], at the beginning of the tenth century B.C., although basing its date on the small palaeographic difference between its letters and those of the other texts mentioned is precarious, given the way the letters are cut in the stones."

[51] Two conspicuous differences vis-à-vis Fekheryeh are that Byblos opted for the archaic legless *kap* while employing the more advanced form (abounding already in late Proto-Canaanite) of the pupil-less *ʿayin*, whereas Fekheryeh picked out the *ʿayin* with pupil, using it concurrently with a legged *kap*, yet straight-legged instead of diagonal-legged, perhaps an 'eccentric' feature, as noted on p. 43.

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- The Elibaal and Yahimilk *waw*s seem to be the most archaic—they could be less developed than Ahiram—and Šipiṭbaal's are the most advanced.
- The Yahimilk, Ahiram and Abibaal *mem*s are intermediate between the more archaic Elibaal example and the advanced Šipiṭbaal shapes.
- The Yahimilk and Ahiram *nun*s are intermediate between Elibaal and Šipiṭbaal.

If this evidence (admittedly slight) is taken at face value, Yahimilk, followed by Ahiram and perhaps Abibaal,[52] would be interposed between the seemingly earlier Elibaal (no advanced letter forms) and the later Šipiṭbaal (greatest number of such forms). Except for keeping Šipiṭbaal's position as last, this order bears no resemblance to the sequence of Byblian kings proposed by Albright (p. 20 with note 12) and challenged by no one except Wallenfels. It reverses the 'traditional' order—founded on the sequence of the pharaohs on whose statues their inscriptions are written—of Abibaal and Elibaal (see pp. 16–17). Nor is the sequence *KAI* 4, 6, 7, founded on the Yahimilk–Elibaal–Šipiṭbaal genealogy in the latter and retained even by Wallenfels,[53] reflected in the above palaeographical order. This order would imply that Yahimilk of *KAI* 4 is not the grandfather, mentioned in *KAI* 7, of Šipiṭbaal, nor the one whose name is habitually restored as Elibaal's father in *KAI* 6. Similarly, Elibaal of *KAI* 6 would have reigned at least two generations before his namesake, Šipiṭbaal's father, mentioned in *KAI* 7. We may have before us recurring dynastic names, the above documentation potentially increasing the Byblos king list by three—two more Yahimilks, one more Elibaal. (For Wenamun's Zakarbaal see p. 17; for Ittobaal, note 55.)

What may be gleaned from the above? The unfeasibility of achieving an inner order founded on the letter shapes seems to impose itself.[54] The differences between the script of Elibaal on the one hand and Yahimilk, Ahiram and Abibaal on the other remain minimal, for the uniform archaizing shapes are the vast majority. Even the seniority of Elibaal implied above may thus be a phantom: How can one tell, for instance, whether this position would have been retained if two missing key letters, *he* and *qop* (cf. Table 2), had been present in Elibaal? What clearly transpires in any case is that the 'traditional' position of Ahiram as first has no support in the evidence, as noted (see also pp. 20–21, 33). The long-held view of a

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[52] Abibaal's position in relation to the former two remains unknown—the inscription contains too few diagnostic letters.

[53] The order arrived at by Wallenfels (1983: 111) was: (1) Yahimilk, Elibaal, Abibaal (Y preceding E, position of A unknown); (2) Šipiṭbaal; (3) Ahiram.

[54] Indeed, other Byblos letters blur the picture further: Judging by the *zayin* (see Tables 1 and 2), Šipiṭbaal would become as archaic as the rest, while Ahiram's graffito would turn out to be the only advanced Byblos inscription. And bringing in the *qop*s could stand the above sequence on its head, with Šipiṭbaal more archaic than Yahimilk. All this accentuates the artificiality of the script and the futility of the attempted inner dating by palaeography.

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common scribal tradition for all four inscriptions is certainly correct. It may well be that once monumental alphabetic inscriptions appeared in Byblos, a standard was set, which several subsequent generations emulated as best they could.

Thus, even though the developed shapes in the fifth inscription—of Šipiṭbaal—confirm its absolute date in the ninth century or the first half of the eighth, they do not necessarily indicate the inscription's relative position: Rather than making Šipiṭbaal last among the five Byblos kings, these letters could simply testify to the greater success of the writers of the four other texts in imitating older script forms.

I turn now to the chronological position of the Byblos inscriptions among the alphabetic texts in the Levant. Sandwiched as they are between the less-developed script of the Tekke and Kefar Veradim bowls of the first half of the ninth century (pp. 34–39) and the advanced script of the Kulamuwa inscription, etc., of its second half (p. 83), there is almost no time left for the Byblos inscriptions if a strictly linear development of the alphabet in monumental inscriptions is assumed. Indeed, if my understanding of the evidence (p. 52) is accepted, such a development is unlikely. All this illustrates once again what has already been amply demonstrated by Fekheryeh, namely that one should be on one's guard against the mechanical application of letter typology for dating inscriptions cut in hard surfaces.

1.3.4.2 Absolute chronology: the beginning and end of the Byblos sequence

The five Byblos inscriptions mention six to nine kings,[55] who by a minimum estimate will have reigned several decades, and possibly about a century. It seems that none of these kings can be tied to the absolute chronology of Egypt or Assyria. (Two hypothetical, very uncertain links for Elibaal and Šipiṭbaal are mentioned in notes 56 and 58.) As noted above, the non-archaizing letter forms in the Byblos inscriptions are too few to establish with certainty the inner order of the royal authors, even Elibaal's position as first or Šipiṭbaal's as last. And—what we are concerned with at this point—there is no hard data on the absolute dates of the beginning and end of the sequence as a whole. Likewise, parallels to Ahiram's reliefs point only in a general manner to the ninth–eighth centuries (pp. 21–22, 75–77).

If the available evidence does not afford a precise dating, it may at least lead to a time range, which I propose was *ca.* 900–750 BCE: A beginning no earlier than *ca.* 900 is derived from the time assigned to the transition from Proto-Canaanite to the Phoenician–Aramaic script, *ca.* 900–850 BCE. Dating the last Byblos inscription to *ca.* 750 at the latest rests on the palaeography of the non-archaizing letters (see pp. 33–34) as well as on my assumption that the actual time of the writing of the

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[55] See p. 47, and assuming that despite not calling himself king on his father's sarcophagus, Ittobaal succeeded Ahiram on the throne.

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earliest of the inscriptions is unlikely to be more than several decades later than the ductus it emulates. This in turn follows the Fekheryeh example (p. 52).

A beginning between 900 and 850 BCE, hence an end between 800 and 750 BCE (according to the 100-year assumption), is my best conjecture for the date of the Byblos inscriptions as an ensemble. Admittedly, this is founded only to a small extent on solid evidence, for there is very little of that. In the following the highest and lowest possibilities are assessed; my preference is for the latter.

If the Byblos inscriptions range in time from ca. *900 to 800 BCE*, the earlier ones would have been written in the most advanced script of their day,[56] contemporary with the more conservative writing on the bowls and late arrowheads; the later Byblos inscriptions would in that case be archaizing, contemporary with the more evolved script of Kulamuwa, etc.

If, on the other hand, the Byblos inscriptions range in time from ca. *850 to 750 BCE* and thus begin at the end of the transition from the Proto-Canaanite to the Phoenician script,[57] all of them have to be archaizing—some more successfully (especially Elibaal), some less (especially Šipiṭbaal).[58]

In presenting these two alternative ranges for the Byblos inscriptions, I hope that two extremes are avoided: positing a strong archaism in the eighth–seventh centuries on the one hand (Wallenfels) or a beginning in the tenth century on the other ('traditional' or high chronology). The former assumes the archaization harks back about two centuries, and this is definitely not supported by the few non-archaizing letters present (see p. 33). The latter is firmly ruled out by the Nimrud parallels to Ahiram's reliefs and by the ninth-century archaeological contexts of the two bronze bowls with Proto-Canaanite inscriptions, as noted.

But is one of these time ranges likelier than the other? I prefer the 850–750 BCE alternative and from this point on use it as the basis of my working hypotheses: The seniority of Elibaal's script may well be a phantom, as noted on p. 47. And as long as no monumental West Semitic inscriptions are dated historically to before

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[56] If the lack of non-archaizing and 'eccentric' shapes in Elibaal (the only such Byblos inscription) is of chronological significance this inscription would be written in the very earliest Phoenician script (i.e., 'post-Proto-Canaanite'), and Elibaal might be regarded as first among the five royal authors. This might make him a contemporary of Osorkon I and a predecessor of Abibaal (see note 9 on the uncertain absolute chronology)—a transposition of the 'traditional' order of these two Byblos kings (cf. p. 47). But I prefer the 850–750 range for the Byblos inscriptions, which has no room for the above scenario.

[57] On the transition see p. 52; on the earlier source of this script see p. 50.

[58] In this case the identification of Šipiṭbaal of *KAI* 7 with the king of Byblos mentioned by Tiglath-pileser III (744–727 BCE) becomes a possibility, first raised, only to be rejected, by Benjamin Mazar in 1946 (178–179 = 1986: 244–245). Wallenfels (1983: 111) considered this identification of Šipiṭbaal a certainty. I do not think it is: surely the 850–750 BCE time range has room for an earlier namesake.

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ca. 850/830 BCE, there is no guarantee that such early inscriptions existed at all (cf. pp. 55–60).

1.3.4.3 The Byblos (and Fekheryeh) script: a possible source

What could have been the source of the Byblos script? If, as I believe, all five inscriptions were carved after the middle of the ninth century, many of their archaizing letter shapes probably reflect the earliest Phoenician–Aramaic script of the first half of the century.[59] The memory of such an 'official' script may otherwise be preserved only in the even more archaizing Fekheryeh ductus. Should the notion that the early-ninth-century script imitated by Fekheryeh and Byblos was copied from monuments be accepted *a priori*? Not at all; as long as no monuments of that time are known, I prefer to seek the source in official administrative (or temple) documents written on papyrus or leather (see further p. 58).

1.4 THE WEST SEMITIC ALPHABET *CA.* 1150–850 BCE AND ITS CULTURAL AND POLITICAL BACKGROUND

The role of the Aramaeans in the transition from Proto-Canaanite to the next stage of the West Semitic alphabet seems to me to have been at least as prominent as the role of the Phoenicians. Indeed, some of the processes with which this section is concerned may well have begun with the Aramaeans.

1.4.1 WEST SEMITIC WRITING *CA.* 1150–850 BCE

1.4.1.1 Introduction

I suggested above (p. 48) that the ninth century could accommodate three stages of the West Semitic alphabet—the last Proto-Canaanite inscriptions on the bowls and late arrowheads in the first half of the century, the Byblian stone inscriptions[60] and the Tel Dan and Mesha steles, etc., in the second half of the century. How could all these script phases, long accepted as spanning a quarter millennium, be accommodated in just one century?

I believe a framework for such rapid progress existed: Palaeographical development is considered a slow and gradual process (see, for instance, Naveh 1982: 7). The rate of change may indeed have been reasonably constant during much of the first millennium, the chief time-span covered by Naveh's book. But should a constant

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[59] On the perception of an initial uniform Phoenician–Aramaic script and the problems it raises see note 87.

[60] Or more exactly the ductus represented by the archaizing majority of the Byblos letters (above).

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pace everywhere and at all times be posited *a priori*? I think not; certain factors could have influenced it.[61]

1.4.1.2 New West Semitic chancelleries and the Phoenician–Aramaic and Hebrew scripts

From *ca.* 1150 to *ca.* 900 BCE, when most West Semitic political formations that originated in the Late Bronze had already become defunct, and when most of those known in Iron II had not yet been born, official use of the alphabet would have been minimal.[62] Under such circumstances it is no wonder that the letters developed at a slower pace and that no regional variants have been discerned.

Then came a turnabout. The ninth century saw the foundation of many a West Semitic state and its royal city. More governmental institutions were certainly formed in the course of this century than all those that existed in the West Semitic world in the preceding quarter millennium. Scribes, no doubt, were in great demand and many were in training. And whereas direct evidence for the extensive application of the alphabetic script in daily affairs in the first half of the ninth century is lacking, the incipient West Semitic administrations are unimaginable without it.[63] It stands to reason that the said surge in official scribal activity produced mainly papyri (and possibly leather) written in ink, all of which are now lost (cf. Millard 1979: 616; Lemaire 1985: 116–123).[64]

This avalanche of documents, I believe, brought about a unique quantum leap in the change-rate of the West Semitic letters:[65] As a result of intensive use, the script would have become cursive almost immediately, and it is in the cursive that the branching off of the 'national' scripts occurred. Within just a few decades the alphabet evolved from late Proto-Canaanite to the Phoenician–Aramaic and Hebrew scripts.[66] The changes, as is well known, also included the stabilization of right-to-left writing, elimination of mirror-image letters and—beginning in the Aramaic script—*matres lectionis* (on these three developments, and on others,

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61 Referring to a slightly later period Naveh (1982: 84) expressed a cognate view: "On account of its international status, the Aramaic script developed rapidly" (more *ibid.*: 97–99).

62 Archaeologically the same situation is reflected in the lack of public buildings and monuments, save during a short recovery towards the end of the period (see p. 67).

63 The use of Assyrian and Luwian in the bureaucracy of the Aramaean kingdoms in northern Syria is less likely, although the persevering of these scripts in monuments of the same kingdoms is documented (see pp. 56–57).

64 Lemaire summarized the largely indirect evidence—one or two later Iron II papyri and numerous bullae that sealed now-lost papyri, Persian period papyri, and textual and pictorial references.

65 A picture comparable to a certain extent—speedier development in the thirteenth (and fourteenth?) century following an earlier 'stagnation', was noticed in the Proto-Canaanite alphabet and attributed to its increased application (Sass 1988: 135, 140). That the alphabet was adopted in Ugarit during the later phase is certainly no coincidence. See Addenda, p. 183.

66 Finally, some time during the second half of the ninth century, palaeographical development stabilized at the sedate tempo documented so well throughout much of the first millennium.

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see Naveh 1982: 42—'traditional' chronology). *Matres lectionis* are documented for the first time in the Fekheryeh inscription of the second half of the ninth century.[67] Whether they had already been introduced in the first half of the century is still unknown; I would not be surprised if they were. (On the mixed Proto-Canaanite and Phoenician ductus of some of the arrowheads and of Byblos cone A see p. 44).

The development of the cursive in administrative documents in due course influenced the shapes of the letters in monumental inscriptions cut in stone (p. 58),[68] and it also gave rise to the unofficial cursive writing. Eventually the unofficial script also influenced the official (Naveh 1982: 7–8) but not, it seems, as early as the transition from Proto-Canaanite to the Aramaic–Phoenician and Hebrew scripts.

In sum, the Phoenician–Aramaic and Hebrew scripts belong to one and the same phenomenon and came into being practically at the same time—*ca.* 900 BCE (on Phoenician–Aramaic see further note 87).

1.4.1.3 The galloping 800s: from Proto-Canaanite via Byblos to Dan and Mesha in less than a century

The first half of the ninth century is, therefore, to be regarded as a transitional period when inscriptions like those from Tekke and Kefar Veradim coexisted with texts written in the ductus of the Byblos stone inscriptions—the former unofficial in character, hence conservative (i.e., Proto-Canaanite) in shape, the latter official and, therefore, written in the more innovative Phoenician script. (It is suggested in pp. 49–50 that the Byblos inscriptions themselves are later, and archaizing.) The latest Proto-Canaanite inscriptions may still have a few mirror-image letters and dotted *ʿayin*s, which the Byblos texts no longer have, but otherwise the scripts are similar.

The contemporaneity of the Tel Dan stele and the Tell Fekheryeh statue in the second half of the ninth century should be understood differently. The former was written in the 'classical' script of its time while the latter was primarily written in a late Proto-Canaanite type. In this case both inscriptions are official, and they differ in that the less evolved Fekheryeh script is a deliberate archaism, possibly imitating the earliest type of alphabetic script employed by the Aramaeans (pp. 50, 58). Yet according to the present suggestion the Fekheryeh archaism is not one that harked back 250 years ('traditional' chronology) but only several decades.

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[67] On possible Assyrian roots of the *matres lectionis* see for instance Segert 1963: 46–47, Zadok 1982: 121 and Andersen and Freedman 1988: 14.

[68] Cf. Naveh 1970b: 10; 1982: 7, 84. Cursive influence in the monumental inscriptions of the second half of the ninth century (and in some other texts carved in hard surfaces) can be detected in the shapes or top-left inclination of no few letters, such as *he* (Kulamuwa, Dan, Fekheryeh), *waw* (Kulamuwa, Dan), *zayin* (Hazael frontlets), *kap* (Kulamuwa gold ferrule) and *taw* (prevalent) (cf. Table 2 on pp. 24–25).

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1.4.1.4 The linear alphabet in Phoenicia and Syria ca. *1150–850 BCE, and Iron I antecedents*

A phenomenon that has perhaps received less attention than it deserves is the absence in Phoenicia (and in Palestine north of the Jezreel Valley) of Proto-Canaanite inscriptions that predate the arrowheads, and in Syria of Proto-Canaanite inscriptions in general. In the whole of Syria the earliest datable inscriptions in the linear alphabet belong to the second half of the ninth century. The documentation of the Proto-Canaanite-to-linear-alphabetic continuity is essentially limited to the Jezreel Valley and southwards (see pp. 54–55). On the other hand, at the time of the arrowheads very few alphabetic inscriptions are known from south of the Jezreel Valley.

I now turn to the three forerunners of the linear alphabet in Phoenicia and Syria: the cuneiform alphabet, the Byblian pseudo-hieroglyphic and the Luwian hieroglyphic.

The Cuneiform alphabet

This script spread as far as the Jezreel Valley, with only one inscription found further south. This dividing line was noticed in the past (e.g., Sass 1985: 228 = 1988: 162) and I now attempt to address the possible implications: Most examples of alphabetic cuneiform found outside Ugarit and Ras Ibn Hani come from unclear contexts, and it is solely on the basis of Ugarit's end shortly after 1200 BCE that scholarship hitherto confined them to the thirteenth–early twelfth centuries (cf. Sass 1988: 166).[69] Did this script in fact continue in use? Is it possible that the writing employed in parts of Syria (south of Ugarit) and Phoenicia in the twelfth–tenth centuries was alphabetic cuneiform which had outlived the end of Ugarit by two centuries and more before being replaced by the linear alphabet? In this connection the observations of Greenstein and Bordreuil (references in Sass 1988: 113, 164) on the Phoenician language of the Sarepta handle should be mentioned. In parts of northern and central Syria the cuneiform alphabet may alternatively have been replaced first, in the tenth century, by Luwian, until Aramaic in the ninth century gradually ousted any earlier script.

What has come down to us of the cuneiform alphabet south of Ugarit are only a few short texts, mostly on metal objects and pottery vessels, quite like the meagre yield of the Proto-Canaanite inscriptions from further south. But why are these finds so meagre? And why do they include almost no tablets? Millard, developing an observation made by Schaeffer, reasoned that the Proto-Canaanite output of southern Canaan was similar to that of Ugarit in alphabetic cuneiform, and that it was only the survival rate of clay tablets versus papyrus that accounts for the great difference in the number of epigraphic finds:

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[69] A fresh study of the archaeological contexts of these inscriptions is now needed.

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> The shorter cuneiform alphabet, although far more widely distributed, seems to have had a lower level of use.... The use of the shorter cuneiform alphabet, as it appears to us, was rather similar to the use of the [Proto-]Canaanite alphabet.
>
> (1991: 101–102)

> Knowledge of the Canaanite alphabet comes from just such a motley array of inscribed objects as those on which Ugaritic was occasionally written..., or from such oddments as those that present the shorter cuneiform alphabet to us. How differently would the handbooks describe Ugarit, how much thinner would be the history books, had we those few objects alone! Would anyone guess the range of the archives of Ugarit on such a basis, or gauge the versatility of her scribes? Ugarit provides a valuable analogy from which the nature and extent of early Canaanite alphabetic documentation may be conjectured.
>
> (1979: 616)

I wish to take this conjecture a step further: The nature of the epigraphic finds raises the possibility that the cuneiform alphabet, when employed for longer documents south of Ugarit, was, like the Proto-Canaanite texts, written mostly on papyri, all of which are now lost.

Byblian pseudo-hieroglyphic

Byblos, as far as writing is concerned, constituted an exception and special case in second-millennium Phoenicia, and also, I believe, in the period with which the present chapter is concerned. The lingering of the pseudo-hieroglyphic script through Iron I until *ca.* 900 BCE,[70] when the linear alphabet finally asserted itself as the sole script in Byblos, may be indicated by one or two overlaps: the bronze spatulae, some of which carry pseudo-hieroglyphic texts and another which has an archaic Phoenician inscription; perhaps also the ruled horizontal lines that several pseudo-hieroglyphic stone inscriptions (Dunand 1945: 71–138) share with Yahimilk (*KAI* 4).[71] And, if my preference for a dating *ca.* 850–750 BCE of the alphabetic royal Byblos inscriptions is accepted, pseudo-hieroglyphic in monumental inscriptions may have lasted in this city even into the early decades of the ninth century, in a manner similar to the initial use of Assyrian and possibly Luwian in Aramaean monuments of the North (pp. 56–57).

The linear (late Proto-Canaanite) alphabet

My above hypotheses attempt to explain the practical absence of pre-tenth-century Proto-Canaanite inscriptions from Phoenicia and northern Israel; the earliest such

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[70] Cf. Sass 1985: 102 with note 27 = 1988: 86 with note 58, still using the 'traditional' chronology.

[71] The ruled lines are one phenomenon linking the Pseudo-Hieroglyphic and alphabetic texts about which Martin's observations (1961: 65) may be valid after all—compare p. 78.

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texts are on the arrowheads and on a few contemporaneous objects (pp. 43–44). The Hazor and Sarepta sherds, hitherto considered Late Bronze Age Proto-Canaanite inscriptions from that area, may at most be regarded as exceptions to the rule; alternatively, they may be irrelevant to the issue in question. In Sass 1988: 71–72, only a late LB date was considered. On reflection this is far from certain: Both sherds are unstratified. The one from Hazor, unearthed in the lower city, which was destroyed in the thirteenth century, has at any rate a *terminus ad quem*. Yet this tiny sherd with its painted coil and cross, hitherto considered clear Proto-Canaanite *lamed* and *taw*, may carry remnants of figural decoration instead.[72] And as to the Sarepta sherd, it belongs to a local painted vessel—judging from the published photograph the tenth century may be possible as well.[73]

1.4.1.5 Elusive beginnings: The Phoenician–Aramaic (and Hebrew) script ca. *900–850 BCE*

For our present purpose the key factor to be reckoned with is that the initial Phoenician–Aramaic (and Hebrew) writing of the first half of the ninth century is still largely inferred. Not even one such inscription is attributable to that time by other than palaeographic criteria:[74] The earliest historically-dated monumental inscriptions—Kulamuwa, Dan, etc. (p. 83)—belong to the second half or last third of the ninth century. The earliest non-monumental texts—the Dan and En Gev sherds, etc. (*ibid.*)—were 'traditionally' assigned to the tenth–ninth centuries. But I believe that if the absolute chronology of their archaeological contexts is re-examined, these texts will turn out to belong to the ninth century, some perhaps to the early eighth.

Taking the archaizing ductus of the Byblos stone inscriptions to be representative of the elusive Phoenician script of the first half of the ninth century, even if the inscriptions themselves were written later (pp. 49–50), I should remind the reader of the two alternative time spans, either *ca.* 900–800 or 850–750 BCE, proposed for the Byblos texts. Each of these periods would yield a very different picture of the advent of the monumental West Semitic inscriptions. It is indeed the less likely picture that emerges from a 900 BCE beginning which led me to prefer the 850 one (see below on the southern Levant). The following reconstruction of alphabetic developments *ca.* 900–850 BCE begins with the monumental inscriptions.

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[72] A painted circle, coil, cross and wavy line on two tiny sherds from the 2002 season at Megiddo caused momentary excitement, until, after mending, these patterns proved to be elements in the decor of their vessel.

[73] Two references that should be added to Sass 1988 are Pritchard 1988: 12 (no. 8) and Khalifeh 1988: 2. Neither adds new data, but the latter re-affirms the hopelessly disturbed stratigraphy of the Sarepta excavation area where the sherd was found.

[74] The Tekke and Kefar Veradim bowls have clear early-ninth-century archaeological contexts, but their more conservative script still belongs to the previous phase—late Proto-Canaanite—in this transitional period.

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The parentage of the West Semitic monumental inscriptions

Luwian display inscriptions were 'always' there, and the Assyrians themselves were inspired by them.[75] Yet it is also true that the Assyrians joined in vigorously during the early part of the ninth century. In this way the debate on whether it was a Neo-Hittite or Neo-Assyrian archetype (Teixidor 2001: 670 versus Na'aman 2000: 94–95) that gave rise to the first Aramaic monumental inscriptions and played a role in shaping their contents loses its heat.[76] What clearly transpires in any case is the northern ancestry of the monumental West Semitic inscriptions (more below).

No monumental West Semitic inscriptions of the first half of the ninth century 1: the Jezireh

That monumental art and inscriptions should be interlinked might seem obvious. Yet as noted time and again in this chapter, the earliest securely-dated monumental inscriptions in the West Semitic alphabet belong to the second half if not last third of the ninth century. The oldest known monumental art of the Aramaeans, on the other hand, is from the beginning of the ninth century or slightly before (and it bears no Aramaic inscriptions): The originally anepigraphic monolith from Terqa (Tell Ashara) on the Middle Euphrates[77] has an added cuneiform text, in which Tukulti-Ninurta II (890–884) mentions his father Adad-nirari II (912–891).[78] In Aramaean Gozan the contemporary or somewhat later Kapara (on the chronology see pp. 93–95) wrote his monumental inscriptions in Assyrian. The Jezireh and Middle Euphrates are thus the homeland of the oldest-known (non-alphabetic) Aramaean monuments. The earliest historically-dated occurrence of Aramaic script on a Jezireh monument, however, is on the Fekheryeh statue, from the second half of the ninth century (see also note 47). On the possible older source of this script see p. 50 and below.

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[75] On monumental art compare Winter 1983: note 98: "[O]nce it was clear that the reliefs of Suhis II and Katuwas were to be dated prior to Assurnaṣirpal, it became a possibility that Carchemish itself,...could have served as a stimulus for the very development of Assyrian architectural relief." The *status quaestionis* and history of research of the Luwian–Assyrian artistic interdependence are summarized in Aro 2003: 295–296.

[76] See already Lebrun 1993: 20–21.

[77] The principal publication is Masetti-Rouault 2001: 89–133, 136. See also Winter 1975: 140; 1982: 365; Mazzoni 2000a: 41; Gerlach 2000: 241. As the monument is neither a stele nor an obelisk (Masetti-Rouault 2001: 90) I opted for the neutral 'monolith'.

[78] Masetti-Rouault suggested (2001: 128) that the lines left blank at the bottom of the cuneiform inscription may have been prepared for an Aramaic text, ultimately not written. The idea is out of the question, however—a monumental Aramaic inscription by the king of Assyria is unthinkable, certainly as early as the beginning of the ninth century.

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No monumental West Semitic inscriptions of the first half of the ninth century 2: northern and central Syria

There is a comparable absence in the northern and central Levant of monumental West Semitic inscriptions of the first half of the ninth century. The negative results of the intensive archaeological activity in the region for more than a century probably indicate that such inscriptions did not merely elude discovery but that they never existed.

This absence may be partly accounted for by the Luwian origin of some of the ruling dynasties in northern and central Syria. Concerning Til Barsip, Bunnens (1999: 613–614), elaborating on a string of earlier attempts to reconcile the Aramaean and Luwian facets of the evidence with one another, suggested that as early as the tenth and the beginning of the ninth centuries ('pre Ahuni' period) an Aramaean dynasty had taken over, yet in terms of monuments still expressed itself in a purely Luwian (more precisely Carchemishean) fashion.[79] This interesting proposal may perhaps apply even better to the different conditions of the first half of the ninth century over a larger area: Could a view of Luwian as the kultursprache have initially led more Aramaean rulers in northern Syria to compose their monumental inscriptions in the hieroglyphs of that language even while already employing the Aramaic script in day-to-day governmental affairs? (See below on latter.) In any case, the use of an older, venerated foreign language on solemn occasions is abundantly paralleled worldwide. On the other hand not every Luwian-writing ruler in the said region should be suspected of having been an Aramaean in disguise.[80] Only a demonstrably Semitic royal name written in Luwian hieroglyphs, equivalent to the names of the rulers of Gozan in the Assyrian inscriptions on the Tell Halaf reliefs, can confirm Bunnens's deduction.

No monumental West Semitic inscriptions of the first half of the ninth century 3: the southern Levant

The lack in the southern Levant in the first half of the ninth century of monumental alphabetic West Semitic inscriptions may in turn be explained by the absence of a model: There existed neither second-millennium predecessors[81] nor an early-first-millennium south Levantine prototype in a West Semitic or other script. On the possible exceptional role at Byblos of the pseudo-hieroglyphic inscriptions see p. 54, yet an influence outside Byblos of this highly localized phenomenon is unknown

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79 Hawkins (references in Bunnens 1999: note 52; see also Hawkins 2000: 225 with note 14) vacillated between an Anatolian, a Hurrian and a Semitic origin of the rulers' names; most recently (2003: 148) he opted for "Semitic and/or Hurrian."

80 Aro (2003: 282) formulated this as follows: "[I]t seems clear that very many, if not all north Syrian sites…bear mixed [Luwian–Aramaean] ethnicity…. But despite the complexity of the question of ethnicity, race and cultural identity,…[i]t is impossible to understand how this language [Luwian] could have flourished and developed in northern Syria for such a long period if nobody spoke it as their mother tongue."

81 The Ramesside inscriptions in Palestine, Phoenicia and Damascus hardly count in this respect (compare Na'aman 2002: 93–94).

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and quite unlikely. Monumental texts in alphabetic script in the southern Levant, documented since the second half of the ninth century, e.g., the Dan and Mesha steles,[82] thus do not seem to be a local development; in content these steles follow Luwian and Assyrian patterns (references above). They may rather be understood as a speedy response to a North Syrian stimulus.

From the specific case of the monumental inscriptions I turn to alphabetic writing in general.

No non-monumental 'post Proto-Canaanite' texts of the first half of the ninth century, or the emergence of the Phoenician–Aramaic script[83]

It is suggested that the transformation from Proto-Canaanite to the next stage of the script came to pass in the official documents of the early Iron II states (see pp. 51–52). Evidently the new cursive was employed mainly on perishable papyrus, or leather, and this is why actual examples are practically non-existent. Inscriptions on durable material such as pottery vessels are likewise rare in the ninth century, and for the first half of the century are not securely attested (see pp. 55, 83–84). We thus learn what this script looked like chiefly from indirect or archaizing evidence: The 'hand' of the majority of the Byblos letters reflects a phase shortly after the transition from Proto-Canaanite even if the inscriptions themselves were written later, and the Fekheryeh ductus may recall the transition itself.

But is it correct to deduce the earliest Phoenician–Aramaic administrative cursive from a later, archaizing, monumental source? I see no difficulty in this regard: In the West Semitic alphabet of the early first millennium the scripts of the monumental and non-monumental inscriptions were sufficiently similar.[84]

Where in the Levant did the Phoenician–Aramaic cursive script emerge? And did the transformation of the Proto-Canaanite alphabet begin in a single centre or simultaneously in several places? I suppose, although it cannot be proven, that it was a single centre that led the way. One point at any rate seems to me quite certain: The contention of the 'traditional' chronology that the transformation of the alphabet *must* have come about in Phoenicia, and a century earlier than anywhere else at that (i.e., the 1000 BCE dating of Ahiram), has no basis in the archaeological and epigraphical evidence. And regarding the first half of the ninth century, scribal activity in the Aramaean centres (and in Israel) would have been no less intensive than in Phoenicia. Is it not equally plausible, then, that the massive new output of

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82 On the Cyprus and Nora inscriptions see note 30.

83 On Hebrew see pp. 59–60.

84 On the absence in the early first millennium of a true West Semitic lapidary style see for instance Naveh 1982: 7, 69, 84. This is why the terms 'monumental' and 'non-monumental' are employed here—the former largely a variant of the cursive script (see p. 52 with note 68).

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administrative documents that ushered in the new ductus and the loss of the last Proto-Canaanite features in the alphabet *ca.* 900 BCE began in a chancellery of an Aramaean royal city?

A conceivable locale for this process can perhaps be singled out. If the pioneer is sought among the Aramaeans, the northern and central Levant (as far south as Hamath) are less likely milieus in light of both the strong Neo-Hittite element in these regions and indeed the lack—in contrast to the southern Levant—of earlier linear alphabetic inscriptions (see p. 53). And so Damascus may come to the fore. First, this capital city was beyond the sphere of Luwian writing. Second, it is not only close to Palestine and Phoenicia, where the Proto-Canaanite alphabet was in earlier use, but it may have had a late Iron I tradition of its own in this script, albeit a short one:[85] I will not be surprised if future archaeological finds from the area of Aram Damascus and/or present-day north-eastern Israel eventually show that the earliest alphabetic notation of Aramaic and the beginning of the cursive were linked to the tenth-century configuration dubbed 'New Canaan' (see p. 67). All the same, the earliest use of the twenty-two-letter alphabet obviously cannot be ascribed to the Aramaeans—the reduction from twenty-nine–twenty-seven letters to twenty-two probably came to pass among the speakers of a Phoenician dialect or dialects (references in Sass 1988: 163–164, 166). Early-first-millennium Aramaic (and for that matter Hebrew, Judahite at any rate) evidently had a richer consonantal repertoire so that orthographic compromises were necessary (see note 199).

Returning to the transformation of the Proto-Canaanite alphabet, my working hypothesis thus regards Damascus as the starting point of the Aramaic cursive. A similar need for governmental writing among all new West Semitic states led to the prompt spread, *ca.* 900 BCE, of this script among the Aramaeans, and to the branching off of the Phoenician and Hebrew counterparts (on Phoenician cf. note 87).

For central and northern Syria this innovation was probably of even greater significance: the initial adoption of the new Damascene ductus for extensive use in the young administrations,[86] if my reconstruction on pp. 53–54 is accepted, coincided in these regions with the introduction of the linear alphabet itself. (This may well be the 'pioneering' ductus which the late-ninth-century Fekheryeh inscription sought to recreate with close to full success.)

The Hebrew script, to deal with it only briefly (more on pp. 85–88), is another descendant, one developed in Samaria, of Proto-Canaanite. Like Phoenician and

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[85] Palestine had an uninterrupted Proto-Canaanite tradition; Phoenicia (and possibly Damascus) seems to have had only a decades-long use. But if the hypothesis on pp. 53–54 is followed, Phoenicia and coastal Syria had a longer prior presence of the short cuneiform alphabet.

[86] Despite the influential Luwian tradition among the northern Aramaeans, a tradition which at this early stage may have found its expression in the perpetuation of hieroglyphic Luwian on monuments (see main text).

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Aramaic writing it would have answered the need created by the emerging Israelite state and its bureaucracy.[87]

The Phoenician–Aramaic (and Hebrew) script* ca. *900–850 BCE—Balance

The absence of monumental 'post Proto-Canaanite' texts of the first half of the ninth century seems to reflect phenomena distinct from the non-monumental:

The lack of *monumental* inscriptions is probably to be taken at face value. After so many years of archaeological discoveries in the Levant the absence of historically-dated pre-850/830 BCE examples can no longer be attributed to the chances of discovery alone; it may in effect represent a true void: Among the Aramaeans of the north the lack of such inscriptions could indicate the initial use of Assyrian or Luwian on monuments; in the southern Levant it might represent a veritable absence of this medium.[88] At the present state of our knowledge I thus prefer to assign the debut of monumental inscriptions in the linear alphabet, including the earliest among the (archaizing) Byblos inscriptions, to the second half of the ninth century.

The lack of *administrative* documents written on perishable papyrus or leather, on the other hand, is only to be expected in the Levantine climate, although the existence of such manuscripts can be deduced with reasonable certainty from indirect indexes (see p. 51). I thus regard the emergence (under possible Damascene lead) of the Phoenician–Aramaic and Hebrew scripts and their subsequent swift, extensive diffusion in the respective administrations as early as the first half of the ninth century as a reality in its own right.[89] Indeed, my conclusion (above) was that these scripts are part of the same phenomenon and came into being at practically the same time.

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87 Naveh's observation (1982: 80) that in the ninth century the Aramaic and Phoenician scripts were practically alike, the differentiation beginning only in the eighth century, is universally accepted. However the question about why these two scripts remained similar has not been satisfactorily answered. Indeed the distinction of the Hebrew script from the start—and of nearly every subsequent script representing a different language, including later Aramaic and Phoenician themselves—renders discordant the notion of all Phoenicians and Aramaeans following initially, for about a century, the lead of a single lettered centre. I wonder, therefore, whether the perception of a 'Phoenician–Aramaic' script is not wanting; whether the small, mostly monumental sample, and/or the fact that the securely-dated inscriptions begin in the second half or last third of the ninth century, are in the way of a correct understanding.

88 Save Byblos, where the pseudo-hieroglyphic script may have continued in use for monuments into the first half of the ninth century, as noted (p. 54).

89 In other words, a north–south flow of the influence is understood for the monumental inscriptions (and statehood and monumentality in general—see p. 65), but the opposite for the linear alphabet at large (see pp. 58–59).

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1.4.2 The social and political background of the West Semitic alphabetic developments

1.4.2.1 The northern Levant 1: Aramaean statehood in the eleventh–tenth centuries?

The above picture of the alphabet rests on the widespread view of the time around 900 BCE as a turning point for the Levant as well as Assyria. Against this it has been argued—in theory not without reason—that the near-total reliance of absolute Levantine chronology on Assyria may be causing a bias, that the absence of Assyrian documentation for developed West Semitic states during the eleventh–tenth centuries—contrasted with the rich data in the ninth and eighth centuries—is on its own an insufficient basis for denying the existence of such states. And in the same vein, that the absence of eleventh- or tenth-century Assyrian parallels for the Assyrianizing Fekheryeh statue—contrasted with numerous parallels from the ninth and eighth centuries—is an insufficient basis for denying the statue and its inscriptions a pre-ninth-century date.

Proponents of this approach have sought to assign to the tenth century rather than the ninth undated reliefs from the Aramaean centres in northern Syria (and undated Phoenician inscriptions and the Ahiram reliefs). The higher dating for these reliefs was sometimes claimed on the strength of the unbroken LB-to-Iron-II tradition, Hittite to Neo-Hittite, in Melid and other places.

This view is part of a long-standing ambiguity in the scholarly literature: When discussing the said continuity, certain authors in recent decades may not have been explicit enough about when the monumental art of the Aramaean states, like that of Tell Halaf, began to make itself visible in what was erstwhile an all-Luwian domain.[90] Failure to detect Aramaean traits is not an adequate basis for 'aging' Aramaean statehood by a full century.[91]

But the outlook may not be that bleak after all. Other authors, whose reasoning appears to me better founded, have sought to isolate Aramaean traits in what may be termed the Neo-Hittite-to-Aramaean imagery continuum, and link them to social and

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[90] Thus, for instance, Orthmann 1971. Cf. Hawkins's review (1974: 308): "The present work leaves the question as to what innovations or motifs can be attributed to the Aramaeans unanswered and indeed unasked."

[91] Material culture evidently does not always parallel ethnicity. But since accord between certain styles and certain peoples is also known to exist worldwide, falling back on the former argument in order to deny the possibility of distinct Aramaean artistic features (e.g., Schwartz 1989: 281) is not enough. The demonstrable presence of Assyrian, Hurrian, Egyptian and other traits in the monuments of different parts of northern Syria already in the ninth century (I do not deal here with later periods) makes it necessary to be more specific with regard to the artistic output of the Aramaeans.

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political developments.[92] Most of these authors also observed that the states ruled by Luwian dynasties were survivors or offshoots of the Hittite empire system, whereas the Aramaeans prior to the ninth century were still largely in a pre- or proto-state phase—hence pre royal cities and monumental architecture, art and inscriptions (cf. note 99).

Consequently, to argue that the absence of Assyrian eleventh–tenth-century parallels for the Fekheryeh statue is incidental (thus the proponents of a higher Fekheryeh date) may mislead; this absence is as richly grounded in the 'proto-historic' condition of the Aramaeans as it is in Assyria's poor circumstances at that time. Much of the eleventh and tenth centuries was for Assyria a period of distinct decline. Even in the heartland, the Assur–Nineveh–Arbil triangle, there had been little royal building activity or new monuments between the mid-eleventh and mid-to-late-tenth centuries (e.g., Roaf 2001: 359–361). Conspicuous also is the dearth of dated tablets from the mid eleventh to the early ninth centuries (*ibid.*: 362).

Nor can the reasonably explicit image of the Aramaeans that emerges from the Assyrian sources be ignored: Except for Carchemish, Tiglath-pileser I (1114–1076) and Assur-bel-kala (1073–1056) alluded to no royal city or other urban centre between the Habur and Mount Lebanon; the Assyrian army was opposed there only by the agro-pastoral Ahlamu–Aramaeans (for a convenient summary of the sources see Sader 2000: 64–65, with some of it quoted on the following pages).

In addition, it has become ever more clear in recent years that several monuments and cuneiform texts of the late eleventh to ninth centuries from the Habur, Balih and Middle Euphrates regions belong to provincial rulers (or governors, depending on the fluid balance of power between Assur and the said territories), who at times owed only nominal allegiance to the Assyrian crown.[93] Some of these rulers, like the author of the Fekheryeh inscription, were Aramaean. Yet until the end of the tenth century no specific Aramaean traits are evident in the culture of these areas, as well as further west, and it is to this latter time that the emergence of the first Aramaean territorial states should be attributed.

Returning to the Fekheryeh statue, attempts to raise its date, hence the date of its inscription, to the tenth century, founder from an additional angle: The just-

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[92] On Tell Halaf, e.g., Winter 1989: 326, and *passim*; Özyar 1992: 218; Bonatz 2001b: 71–77 (more work is needed in order to evaluate the very different interpretations of the latter two authors). See also note 120 herein.

[93] On the historical setting of these sometimes 'client kingdoms' see for instance Blocher 2001: 308–309; Cancik-Kirschbaum 2000; Harrison 2001: 121–122, 128–129; Masetti-Rouault 2001: 78–87; Postgate 1985: 100–101; 1997: 165; Roaf 2001: 358, and fig. 2: 3.

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mentioned monuments and documents of that century from the same region (some cited in Roaf 2001: note 3), are inscribed exclusively in cuneiform; monuments with Aramaic inscriptions are unknown and indeed unexpected before the second half of the ninth century (see pp. 56–60). The Fekheryeh statue, moreover, even while provincial (cf. note 153), forms part of the flood of Assyrian monumental art that began with Assurnasirpal (883–859). The Assyrian sculptural comparisons to Fekheryeh are thus decidedly *Neo*-Assyrian, attributable quite broadly to the ninth and eighth centuries. Refining this date depends on historical considerations and to a lesser extent on palaeographical ones. For the former (yielding the 840s or 820s) see pp. 93–95, the latter pp. 40–43

With the possible exception of the Til Barsip steles (see p. 57), no references to the Aramaeans were recognized in the hieroglyphic Luwian texts of the tenth–early ninth century. (Some aspects of early Luwian–Aramaean interaction are addressed in Excursus D, pp. 89–92)

In sum, the archaeological and written documentation indicates that in the tenth century, and even more so in the eleventh, most West Semitic political structures were still in a formative stage, which in the case of the Aramaean realm was labelled proto-history (1111–912 BCE) by Zadok. This phase ended "in 912, when Adad-nârârî II ascended the Assyrian throne and started liquidating the Aramean territorial units in the Jezireh" (Zadok 1991: 104–105). The transformation of the Aramaeans "from nomadism to monarchy" will have been the result of several interlinked factors; on two of these see, for instance, Sader 2000: 69, 72:

> Aramaean…control or at least presence on the major Syrian [trade] routes…might have helped attracting more people to settle and would have increased the wealth and power of the settling groups who felt the need to protect the newly acquired land…. [T]he need for a person capable of organising the protection of the settled [Aramaean] territories might have arisen from increasing Assyrian pressure since the late 10th century B.C.

Rather than as invaders, new on the scene,[94] the Aramaeans are rightly understood as a local element in changing social conditions,[95] not unlike the way the early Israelites are viewed.[96] All this transpires from both the archaeological evidence[97]

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[94] This veteran view is still maintained by some, e.g., Peckham 2001: 21: "The Aramaeans, unlike the aboriginal Phoenicians, were newcomers to the region they infiltrated…." Cf. also Orthmann 1993: 250–251.

[95] E.g., Schwartz 1989: 281–283; Sader 2000: 63, 65, 67; Masetti-Rouault 2001: 80–81. On the Amorite roots of Aramaic see Zadok 1991: 107 with earlier references, 114.

[96] Similarly Akkermans and Schwartz 2003: 367 with notes 15 and 16.

[97] See for instance the summary in Sader 2000: 66. The archaeological picture, still rather sketchy, is being filled in by fieldwork such as that described in Hausleiter and Reiche 1999, Harrison 2001, or Wilkinson and Barbanes 2001.

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and the meagre Assyrian documentation that exists after all (above). Indeed, it is on the strength of these considerations that some of the archaeologists working in Syria placed the Iron I–II transition at *ca.* 900 and not 1000 BCE, even before the corrected chronology for south Levantine archaeology advocated the same for that region. Mazzoni (1992: 167), for instance, wrote:[98]

> The Iron Age I in northern Syria seems to be deeply rooted in the Late Bronze Age; a transformation occurs only in the 9th cent. B.C., marking the beginning of local Iron Age II, as it is witnessed, at least at Tell Afis, by the growing size and general replanning of the settlement, now encircled by a defense wall, and finally by the renewal of the pottery technology. When compared to the historical data of the period, this transformation may be linked with the reorganization of the Aramaean principalities when faced with the Assyrian expansion.

1.4.2.2 The northern Levant 2: Aramaeans, Luwians and Assyrians in the ninth century

The first Aramaean royal cities probably emerged not long before 900 BCE.[99] In some of the Neo-Hittite regions where the Aramaeans formed part of the population they had eventually taken over, and it may be conjectured that the earliest takeovers ultimately also led the way for the newly founded Aramaean kingdoms and royal cities. It is thus the late tenth century, or for convenience the round figure of 900 BCE, that should be regarded as the beginning of the new era. Seen in this light, the evolved political situation in the West Semitic world described by Assurnasirpal II (883–859) and Shalmaneser III (858–824) early in his reign will have been the outcome of a process that began in earnest decades, not centuries, earlier. The following quotations conveniently summarize the situation:

> [T]he term Aramaean used in a Syrian or north Mesopotamian context virtually disappears from the Assyrian royal annals after the reign of Ashur-dan II [died 912 BCE], when the names of individual states are mentioned instead.
>
> (Schwartz 1989: note 6)

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[98] See also Mazzoni 1990: 83; 2000a: 56, and *passim*.

[99] "Syro-Anatolian continuity and integration, under the persisting presence of the Luwian dynasties, therefore, are factors which characterize this phase of Iron I.... Its duration would seem to correspond with that of the autonomy of local Syro–Hittite dynasties. The artistic documentation, in fact, gives a uniform picture throughout the tenth century.... A far from secondary aspect of the ninth century is the fact that the Aramaeans become less elusive. The visible Aramaeanization of the region is achieved with the presence of images and celebratory inscriptions."—Mazzoni 2001: 101, 104 (likewise the table, *ibid.*: 102, and Mazzoni 2000a: 56; compare note 102 herein). Yet elsewhere Mazzoni (1994: 334; 1995: 190; 2000a: 31, and *passim*) inexplicably took the Luwian and Aramaean foundations to be contemporary nearly from the start, even while conceding (1994) that 'propaganda' (read 'monumental inscriptions') began among the Luwians in the eleventh century but among the Aramaeans only in the ninth.

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> [F]ortified and royal cities of the Aramaeans, *alâni dannûti* and *alâni šarrûti*, which were completely absent in the earlier sources, appear for the first time in the annals of Ashurnasirpal II at the beginning of the 9th century. Noteworthy is the fact that they are all mentioned in connection with established Aramaean political units which were also unknown before. The emergence of urban centres seems to have either accompanied or immediately followed the unification of a territory.... Since these urban centres and the political unit to which they belong are attested as early as the first half of the 9th century, it is not far-fetched to assume that the formation of the geo-political entities and the subsequent urbanisation process started in the late 10th century.
>
> (Sader 2000: 72–73)

I return to the issue of Aramaean traits in figural monuments: The prevalence of Luwian imagery in northern Syria will naturally have continued into the ninth century where the ruling dynasties were themselves Luwian. It predominated moreover, initially at all events, also among the Aramaean kingdoms which, as is well known, had no prior monumental tradition of their own.[100] Even if the overlaps with the diverse Luwian prototypes remained prominent, with time the Aramaeans may have begun to execute certain details differently.

Just as the political fragmentation of the Luwian-writing area most probably rules out uniformity in the art of all Neo-Hittite principalities (see further p. 91), so it seems that no uniform Aramaean style is to be expected. Yet the striking homogeneity of the late-ninth-century Phoenician–Aramaic script on monuments from Sam'al to Dan (430 km as the crow flies) shows that the picture may be less clear-cut. At this stage I can only put the question of why monumental art and monumental inscriptions behave differently over a geographical distance.

In sum, the debt the early Aramaean kingdoms owe to the Luwian dynasties in the cultural and political domains is a considerable one.[101] The emerging Aramaean powers in the late tenth and early ninth centuries will have emulated Luwian statehood and monumentality extensively. The Assyrian pressure, and model, arrived at about the same time, accelerating the Aramaean transformation.[102] A north–south wave of state founding thus engulfed within a short period most of the Levant and even—if indirectly—Arabia (see p. 118 on the latter). In parts of the southern Levant

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[100] The suggested Luwian preponderance in monumental inscriptions until *ca.* 850 BCE belongs to a different, though obviously related phenomenon (see p. 57) and the conspicuous Assyrian influence on the art of both Luwian and Aramaean royal centres in the ninth–eighth centuries is beyond the scope of this book.

[101] E.g., Hutter 1996; 2001: note 47; Sader 2000: 68, 75; Masetti-Rouault 2001: 5, 80–81; Teixidor 2001: 669–670. On the Assyrian debt to the Luwians see p. 56 with note 75.

[102] On the role of the Assyrians as a catalyst in Aramaean state formation in the ninth century see also Mazzoni 2001: 105, and Na'aman 2000: 95, among others.

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this development may have linked into or replaced a fairly advanced, short-lived local configuration, attested archaeologically late in Iron I—'New Canaan', p. 67

1.4.2.3 Phoenicia and the southern Levant: an Iron I prelude

Some recent studies on the Levant at the beginning of the first millennium, even while admirably up to date on northern Syria, did not incorporate the results of the most recent research when dealing with the southern Levant. Hence, these studies still accord the biblical narratives of David, Solomon and Hiram—and the 'traditional' archaeological interpretation founded on them—the same validity as to primary, tenth- and ninth-century Luwian and Assyrian sources. Inevitably this led to contradictions in the picture of the two halves of the Levant, contradictions which the authors (e.g., Sader 2000: 70–72; Mazzoni 2000a: *passim*; 2000b: note 23) had to leave unsolved. The beginning of Iron II in Syria is usually placed at *ca.* 900 BCE (see p. 64). For Cis- and Transjordan, as long as the dating of strata based on the United Monarchy narratives of the Old Testament was accepted by all, the Iron II revival was assigned to the tenth century. With the corrected chronology (Finkelstein 1996) the same strata were down-dated to the Omride dynasty or its contemporaries, which in turn resulted in a synchronization of the southern and northern Levant (and Greece—see p. 153).[103] The current south Levantine picture and its implications follow.

Well-excavated Dor constitutes a good starting point for evaluating the position in Iron I of the Phoenician or Phoenicianized coastal cities. The relevant levels in E. Stern's excavation testify to a fortified urban centre and developed port, for now with no monumental art or inscriptions, that prospered without serious interruption from the Late Bronze Age through Iron II (Gilboa and Sharon 2001; 2003). The Iron I–II transition at Dor according to numerous ^{14}C dates occurred *ca.* 880–850 BCE (Gilboa and Sharon 2001: 1345–1347; slightly differently 2003: 54, and *passim*; further Sass 2002a: 252). This may seem 20–30 years too low. I believe that the accumulation of more ^{14}C dates and possibly refinement of the dendrochronology of the Levant will clarify the picture before long (see also note 108).

The archaeological data from most other coastal cities in Iron I—even the generally adequate precision and exposure of the Tyre, Sarepta and Beirut excavations—are still silent on this issue. In Beirut fortifications were found which

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[103] In turn, the destructions attributable to Sheshonq's campaign could be an ideal tenth-century anchor for Palestine, had not the relevant strata been disputed practically everywhere; on Arad Stratum XII compare the conflicting conclusions of Herzog 2002: 93: "[N]ot destroyed by Shishak...apparently...used well into the ninth century...," and Singer-Avitz 2002: 114: "Shishak's campaign apparently took place between 930–925..., after which Stratum XII at Arad probably terminated." On the absolute chronology of the early 22nd dynasty see note 9.

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Badre (1997: 60–66) assigned to that time, although Finkbeiner (2002) considered them to be later.[104]

Finally, the passages in the classical authors on early-first-millennium-BCE Tyrian chronology are unusable for our purpose: Like most Hellenistic sources referring to events of great antiquity the passages from Dius and Menander of Ephesus, preserved in Josephus (cf. Barnes 1991: 29–55), should be regarded as "'creative' historiography,...creative in the sense that it refers back to canonical histories [in our case the Bible—*B.S.*], which are changed and embellished in accordance with the realities surrounding the Hellenistic writer" (Mendels 1988: 13). Furthermore, how can the reference to the ancient Tyrian records allegedly used by Dius and Menander be considered more reliable than, say, the correspondence of Hiram with Solomon, a correspondence allegedly kept in the Tyre city archives, quoted by Eupolemus, and likewise referred to in all seriousness (though not quoted) by Josephus (Katzenstein 1973: 78; Mendels 1987: *passim*)?

Despite this insufficient documentation, if the picture at Dor is as described above, it is reasonable to assume that at least some of the Iron I cities further north fared no worse. And if a region in the southern Levant is sought where the Bronze and Iron Age urban cultures were bridged in a manner similar to that of Carchemish and Melid in the north, Phoenicia would be the obvious candidate.[105] The emergence of the Aramaean kingdoms in northern Syria is fundamentally different: As noted, they had no uninterrupted Late Bronze Age political and urban roots.

Regarding another part of the southern Levant, it has been suggested (Finkelstein 2003: 77, and *passim*) that in the lowlands to the southeast of Phoenicia, just prior to the emergence of the first Iron II territorial states in the ninth century, one finds the short-lived 'New Canaan'—the late Iron I revival of Late Bronze type centres. (Fritz [2000] noticed the same phenomenon, but holding on to the 'traditional' chronology obscured his view.) Exactly how southern Syria fit into this state of affairs is still undocumented, yet the archaeological picture around the nearby Sea of Galilee (Finkelstein, *op. cit.*) makes an analogous tenth-century 'awakening' in Damascus likely (on writing see p. 59).

1.4.2.4 Conclusions: the Levant and its alphabets, chiefly ca. *900–850 BCE*

I hope to have shown that the fading out of the Proto-Canaanite script paralleled the social and political processes mentioned above, *ca.* the first half of the ninth century. At that time the first West Semitic territorial states emerged, for which a Luwian and/or Assyrian impetus takes considerable credit, and the Phoenician–Aramaic

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[104] For Tell Kazel (Capet and Gubel 2000: 430, 432) there is evidence of destruction at the end of the Late Bronze Age.

[105] On Carchemish and Melid see Hawkins 1995: 88–92, on writing in Phoenicia—pp. 53–55

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and Hebrew cursive scripts developed and spread in the respective new royal administrations.

But, rather than constituting an even north–south wave of statehood and monumentality, this transformation may have embodied a parallel south–north wave of the alphabetic writing itself. The changes to the West Semitic social and political landscape and to the West Semitic alphabet, that swayed much of the Levant in the course of a few decades in the early ninth century, are thus to be perceived as interdependent in a complex manner that is still insufficiently documented.

In its advocacy of an Aramaean lead and north–south flow of much of the influence, this proposal differs from the 'traditional' picture of a Phoenician preeminence in the transformation of the West Semitic alphabet from Proto-Canaanite to its next stage.

1.4.2.5 Beyond the Levant: The Semitic alphabet and the Phoenician westward expansion

Two Phoenician inscriptions from the West, the Tekke bowl and Nora fragment,[106] have occasionally been assigned to the eleventh century in the scholarly literature—with far-reaching historical consequences. In other works, mine included, these inscriptions and the beginning of the expansion beyond Cyprus are dated to the ninth century, presumably its second half (see note 30, and Lemaire 2004). And whereas the last owner of the Tekke bowl was not necessarily a Semite, the monumental inscriptions from Sardinia certainly testify to Phoenician presence.

1.5 THE WEST SEMITIC ALPHABET *CA.* 1150–850 BCE—SYNOPSIS

While I was working on the early history of the alphabet over twenty years ago,[107] it was Sznycer's 1979 publication of the Tekke bowl with its late Proto-Canaanite inscription that kindled my mistrust in the high dating of the Byblos inscriptions: After many years when there was nothing to tie the West Semitic alphabet *ca.* 1150–850 BCE to absolute chronology (pp. 14–15), the *ca.* 900 BCE archaeological context of the Tekke bowl provided the first such anchor. Looking back, the Tekke inscription alone suffices to undermine the 'traditional' chronology's benchmark 1000 BCE date for Ahiram, and with it the entire edifice. Moreover, just as the pinning down of Ahiram's inscription at that date was to lead palaeographical research astray for decades, so too has the high dating of Ahiram's reliefs (whether to the thirteenth/twelfth century or to *ca.* 1000 BCE) encumbered the study of Levantine imagery.

Yet however uncomfortable I was for a long time about the high dating of the

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[106] Tekke bowl – pp. 34–36; Nora fragment – p. 83 with note 131.

[107] Since my M.A. thesis on the Proto-Sinaitic inscriptions, submitted in 1982.

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Byblos and late Proto-Canaanite inscriptions, my alternative is offered only now, in the wake of two developments of the last decade: the 1996 correction of the chronology of early Iron II south Levantine archaeology and the 1997 discovery of the late Proto-Canaanite inscription on the ninth-century Kefar Veradim bowl. Indeed, the entire picture of the alphabet in the period under discussion, West Semitic and Arabian and Greek, long fraught with seemingly insurmountable difficulties, thus finally falls into place.

1.5.1 Main points recapitulated

In one sentence

The transformation of the alphabet from Proto-Canaanite to the Phoenician–Aramaic and Hebrew scripts occurred in the administrations of the newly-founded West Semitic states at the beginning of Iron II, *ca.* 900–850 BCE.[108]

Other-than-palaeographical means of dating

- The last Proto-Canaanite inscriptions belong *ca.* 900–850 BCE according to the archaeological context of the Tekke and Kefar Veradim bowls, in the case of the latter also the type of the bowl, which can hardly be earlier.
- Art-historical considerations place the Ahiram sarcophagus reliefs, hence the inscription, in the ninth–eighth centuries.
- All other West Semitic alphabetic inscriptions *ca.* 1150–850 have only a palaeographical, i.e., relative means of dating within this range (on the ʿIzbet Ṣarṭah ostracon and Raddana handle see p. 15).

Development and distribution of the linear alphabet

- It was probably only in the course of the tenth century that the linear alphabet spread from Palestine to Phoenicia and southern Syria, and only as late as *ca.* 900 BCE that it was transmitted to central and northern Syria.
- Monumental alphabetic inscriptions seem to have emerged even later, in the second half of the ninth century.
- The origin of West Semitic administrative, cursive writing may be Damascene.
- The royal Byblos inscriptions span about a century, *ca.* 850–750 BCE, yet their archaizing ductus mostly imitates an early-ninth-century hand.

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[108] I presented this conclusion for the first time in 2001 at the annual Aharoni conference of Tel Aviv University's Institute of Archaeology. Incidentally or not, *ca.* 900–850 BCE was also the interim result reached by a team investigating the ^{14}C dating of the Iron I–II transition (lecture by Elisabetta Boaretto, Ilan Sharon and Ayelet Gilboa in the departmental archaeology seminar at Tel Aviv University, 12 January 2004). Time will tell whether this striking agreement remains valid.

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- The said palaeographical situation renders impossible the establishing of an inner order, based on the ductus, of the Byblos kings. The 'traditional' place of Ahiram as first has no support in the evidence.
- The Fekheryeh archaism amounts to several decades (it goes back to *ca.* 900), not 250 years (eleventh century).
- In contexts of the ninth century, excavations north of the Jezreel Valley yielded only Phoenician and/or Aramaic inscriptions, so that doubt is cast on the notion of Omride rule in Galilee and the northern Jordan Valley prior to the eighth century (pp. 83–87).
- Hebrew writing in Judah was adopted practically immediately after its emergence in Israel in the early ninth century (pp. 87–88).

Predecessors of the linear alphabet in the northern and central Levant

- In parts of Phoenicia and Syria the alphabetic cuneiform script may have survived the destruction of Ugarit by about two hundred years, until some point in the tenth century.
- In northwestern Syria in the tenth century, hieroglyphic Luwian could alternatively have been employed also among West Semitic speakers.
- Monumental inscriptions in certain Aramaean royal cities in northern Syria may have been composed in Luwian or cuneiform up to *ca.* 850 BCE, even if the linear alphabet had been employed there for administrative writing since *ca.* 900.
- In Byblos, until the linear alphabet finally asserted itself *ca.* 900 BCE, the pseudo-hieroglyphic script probably continued to be used; in monumental inscriptions it may even have lingered until *ca.* 850 BCE.

1.5.2 THE CHRONOLOGY AND ITS IMPLICATIONS—AN OVERVIEW

The 'traditional' or high epigraphy chronology is based solely on the wish—elevated by Albright (1947: 153) to the rank of decree—that Abibaal and Elibaal respectively were contemporaries of Sheshonq I and Osorkon I; no other foundation exists. Dates that are in reality the highest possible for the two Byblian kings were authoritatively declared the sole acceptable ones. But the resultant fixing of Ahiram *ca.* 1000 BCE and of the late Proto-Canaanite script in the eleventh century goes against the archaeological and other evidence, and has thus led to a series of impossibilities which swelled over the years. It obligated adherents of the 'traditional' chronology to espouse that:

1. West Semitic monumental art of Iron II appears on the Ahiram sarcophagus 100 years and more before it begins anywhere else (pp. 21–22, 75–77).
2. An Iron II type metal bowl appears in Kefar Veradim 100–200 years before it appears anywhere else (pp. 36–39).

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3. A monumental Aramaic inscription appears at Tell Fekheryeh 250 years and more before such inscriptions begin anywhere else (pp. 40–43).
4. The cursive shapes of *he* and *taw* appear in the Fekheryeh and Bordreuil champlevé inscriptions 150–250 years before they begin anywhere else (pp. 32–33, 40).
5. A personal seal with a linear West Semitic alphabetic inscription appears at Revadim 350 years before such seals begin anywhere else (note 130).
6. Phoenician inscriptions appear in the West 200 years before any other trace of the Phoenician westward expansion beyond Cyprus (Tekke bowl and Nora fragment, p. 68).
7. By Semitic palaeography the end of the Greek Proto-Geometric period dates 200 years earlier than what is indicated by all other evidence (Tekke bowl, p. 36).
8. The Arabian alphabet was adopted from the Semitic 300–600 years before the first Arabian inscriptions appear (Chapter 2).
9. The Greek alphabet was adopted from the Semitic 300 years before the first Greek inscriptions appear (Chapter 3).

True, of these misjudgements (as I see them) only one, the ultra-high dating of the Aramaic text on the Fekheryeh statue, has thus far been universally discarded. And if there had been just one or two more of the above problems, they might perhaps still be justified somehow (chances of discovery, heirlooms…). But not, I think, after they multiplied so persistently.

It is on two main points that I disagree with the 'traditional' approach—the high chronology and the wanting differentiation between archaizing, non-archaizing and 'eccentric' letter shapes in monumental inscriptions of the period in question. These two points are evidently interlinked: The dating of the archaizing Byblos inscriptions by their majority of old letter-forms rather than by the few younger forms, together with the wish (or decree) mentioned above, led to the 1000 BCE dating of Ahiram, itself the centrepiece of this approach.

Indeed, in the wake of the ostensible chronological inconsistencies revealed by Fekheryeh, early-first-millennium West Semitic epigraphy in the 1980s and 90s underwent a certain self-criticism, but its results appear to me doubly unsatisfactory: Not only did this activity fail to lead to a rethinking of the two central problems just mentioned (see for instance note 33); it created an impression, certainly among classicists, that the field was erratic (pp. 151–152). I do not think it is: To pronounce the 'traditional' classification and relative dating by letter typology, or palaeography, invalid—this was the verdict of at least two

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Semitists[109]—is in my opinion unduly harsh. And to brand the evidence itself as chaotic is an outright mistake.

When the distinct character of letters cut in hard surfaces (as opposed to ink inscriptions) is heeded and sufficient attention paid to traits denoting artificiality in the script,[110] the points where Fekheryeh and Byblos seemingly contradict relative chronology do find an easy explanation within the 'traditional' typology; but at a considerably lower date.

In other words, much of the 'traditional' *relative* chronology of the alphabet *ca.* 1150–850 BCE—the general line of development, or typology—is to me as sound as ever. It was rather the no-longer-sustainable *absolute* chronology, its increasing troubles rooted not in the evidence but in the paradigm imposed on the evidence, that was in crisis.[111] And it is this chronology with which the present chapter takes issue.

Table 4 contains a sketch of the 'traditional' chronology as well as my past and present views. Listed are the West Semitic inscriptions with a means of dating other than the palaeographical (and the Arabian and Greek alphabets). Excluded because they have only a palaeographical dating are the arrowheads (pp. 43–44) and Nora fragment (p. 83). All the same, their dating, too, is herein considered to be much lower than the 'traditional' chronology's eleventh century.

The dates in the last column for the bowls and the Byblos inscriptions are derived from the following non-palaeographical considerations (by extension, the dating of the Arabian and Greek alphabets is based on them in part):

- The ninth-century archaeological contexts of the Tekke and Kefar Veradim bowls, in the latter bowl also the typological dating of the bowl itself to the ninth century at the earliest. These date the latest Proto-Canaanite inscriptions, hence the transition to the Phoenician–Aramaic and Hebrew scripts, to *ca.* 900–850 BCE.
- The ninth(–eighth?)-century Nimrud ivory parallel for Ahiram's reliefs.
- The proposed link between the transformation of the alphabet from Proto-Canaanite to the Aramaic–Phoenician and Hebrew scripts and the emergence in the first half of the ninth century of West Semitic states and their administrations.

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[109] Kaufman 1986: 3, and *passim*; Lipiński 1990.

[110] I.e., when one cares to look for non-archaizing and 'eccentric' features, few though they may be, that can reveal for a monumental inscription a palaeographical date later than that indicated by most of its archaizing letters.

[111] To apply Lipiński's words to a conclusion the opposite of his.

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TABLE 4. KEY DATINGS OF WEST SEMITIC INSCRIPTIONS

	'Traditional'	*Sass 1980s, 1990s*	*Herein*
Tekke bowl	11th century	"towards 900" (1988: 91)	*ca.* 900(–850?) (pp. 34–36)
Kefar Veradim bowl	[11th century—note 42]	–	*ca.* 900–850 (pp. 36–39)
Byblos inscriptions	10th century	10th century? (cf. note 4)	*ca.* 850–750 (pp. 49–50)
Revadim seal	12th century	9th century (1983; 1988: 93–95)	9th–8th centuries (note 130)
Arabian script	15th–12th century	*ca.* 1000 (1991a: 87)	mid 9th century (Chapter 2)
Greek script	11th century (Naveh)	9th century (1991a: 98)	mid 8th century (Chapter 3)

These links to absolute dates, few though they may be, have to be set against the 'traditional' chronology's total lack of such anchors, a lack addressed above (including the rejection of the purported Sheshonq I and Osorkon I dating of Byblos, and of the 'heirloom excuse').

My chronology differs from the 'traditional' one on the 1150–850 BCE interval only, with repercussions until *ca.* 750. For instance, more of the known inscriptions are crowded together at the end of the period in question, leaving but a thin spread for the preceding centuries:[112] Of the Proto-Canaanite inscriptions attributed hitherto to the twelfth–eleventh centuries most—the arrowheads—go down to the tenth–early ninth century. And so, those texts dated in the past to the tenth century, chiefly the five archaic royal Byblos inscriptions, go down to the first half of the ninth. (This is suggested for the prototype of their archaizing ductus; the inscriptions themselves, as well as Ahiram's sarcophagus reliefs, are wholly or partly later, probably *ca.* 850–750 BCE.)

The archaeological evidence indicates that the transition from Proto-Canaanite to the next stage of the West Semitic alphabet occurred roughly within the first half of the ninth century. It is proposed that when West Semitic territorial states began to emerge *ca.* 900 BCE, official writing—in all likelihood rather limited during the preceding quarter millennium—witnessed an unprecedented surge. It is further suggested that under such circumstances the letter shapes evolved for a few decades

[112] According to the 'traditional' chronology the extant inscriptions cover this period quite evenly.

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with greater speed (accelerated 'cursivization') within the new administrations and chancelleries, giving rise to the Phoenician–Aramaic and Hebrew scripts.

Intriguing questions arise if my views are shared and my chronology is accepted: How, in the first place, could the 'traditional' dating of the late Proto-Canaanite and early Phoenician inscriptions be devised with so much precision and confidence when links from relative to absolute chronology were initially missing altogether? And, moreover, how, once such links finally began to turn up and indicated that the 'traditional' dating was off by some two centuries, were these links so systematically ignored and so profound an error maintained for so long? The answer to this undoubtedly warranted question, I am afraid, has little to do with palaeography directly.

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EXCURSUS A. SIX NOTES ON AHIRAM'S SARCOPHAGUS, ITS RELIEFS, AND THEIR TIME

Some of the conclusions concerning Ahiram's sarcophagus (on pp. 17–22) arise as a result of the following points:

1. A BANQUET SCENE ON AN IVORY PYXIS FROM WELL AJ AT NIMRUD

In her time, Porada (1973) had no exact parallel for Ahiram's banquet scene as an ensemble. For this book, the main relevance of Porada's treatment of the banquet's individual components is that all the comparisons she considered applicable belong to the ninth–seventh centuries. Moreover, her conclusion (1973: 361) on the distance between the Late Bronze Megiddo banquet ivory and the Iron II Ahiram banquet was that "there are differences between the two representations which indicate that the relation is one of subject matter, not style." See further Point 3 on p. 77.

The year after Porada's paper appeared, new ivories were discovered in Well AJ of the North-West Palace at Nimrud, among them a Syrian pyxis with a banquet scene closely resembling Ahiram's (Fig. 5 on p. 21). This was not the first banquet on Iron II ivories, but it is the only one with such a cluster of similarities to Ahiram—the throne of the sphinxes,[113] the distinct, voluted footstool 'brackets',[114] table,[115] and angled fly-whisk.[116]

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[113] That only two legs are visible in the throne sphinx of Ahiram and on a Megiddo ivory (Gubel 1996: 143) is an exception, no doubt reflecting the way four-legged pieces of furniture (often lion-legged) are depicted. Otherwise, second- and first-millennium two-dimensional images of quadrupeds, including the Well AJ throne sphinx, display all four legs.

[114] Such a device is not infrequent in representations of footstools on early-first-millennium ivories, both Phoenician and North Syrian (cf. Gubel 1996: 149, and already Porada 1973: 362). Actual plaques of similar outline were found among the Megiddo ivories (references in Porada 1973: 363) and were occasionally invoked as proof for a Late Bronze beginning of these horizontal elements in footstools, hence a Late Bronze Age date for Ahiram's reliefs. This is unlikely. First, these scroll-like components are missing from Late Bronze Age representations of footstools. Second, the Megiddo items are far too small: The vertical measurement of an actual horizontally-placed footstool bracket would be *ca.* 12 cm (with Ahiram's foot 25–30 cm long, the horizontal measurement of the footstool would be *ca.* 50 cm, the vertical measurement of the brackets about a quarter of this). The Megiddo brackets in turn are only between a quarter and a third of this size (no. 213 = 3.8 cm, no. 214 = 3.9 cm, no. 215 = 3.4 cm). The Megiddo pieces must, then, have served another purpose; they could have been used as facing for the legs of a box, legs similar in shape to those of second-millennium benches, for which see Gubel 1987: 232–233.

[115] With one difference—the Nimrud table has a socle, which is missing in the Ahiram table. Tables and stands with and without this device appear in both the Late Bronze and Iron II, e.g., two socle-less Iron II tables at Karatepe (Çambel and Özyar 2003: Pls. 145, 207).

[116] Misunderstood by Porada (1973: 360). Depictions of diverse fans and fly-whisks are certainly known in Levantine art before Iron II, but, as far as I was able to ascertain, the specific Ahiram shape is not.

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The first to point out this similarity were Muyldermans in 1989 and Herrmann in the same year, yet neither author was led by this comparison to down-date Ahiram from the customary 1000 BCE.[117] Herrmann, moreover, proposed a time-span *ca.* 1100/1000–850 for the ivory pyxis and its group, in line with the link she advocated between the ivories and the Tell Halaf monumental art, and her high dating of the latter.[118] With Irene Winter and several others I doubt the exclusiveness of the Halaf link[119] and follow in any case the available evidence for a lower and shorter life-span, *ca.* 900–850, of the Halaf 'temple-palace' and independent artistic output, in accordance also with the political circumstances (cf. Winter 1989: 324, and pp. 93–95 herein). In other words, Herrmann's dating for the ivories remains far too high even if her Tell Halaf linkage is accepted.[120] And if another north Syrian source for the 'flame and frond' ivories is sought, an end later than *ca.* 850 BCE is not to be excluded (cf. also note 122).

Thus, when the striking similarities—not just general ones but in the small, revealing details—are taken into consideration, they lead to the conclusion that the Ahiram sarcophagus and the Well AJ ivory pyxis are ninth- or eighth-century contemporaries.[121]

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[117] Muyldermans 1989: 393; Herrmann 1989: 90, 94, 105; see also Herrmann 1996b: 155, 159–160. For earlier references to the Well AJ ivories see Herrmann 1989: note 7, to which Barnett 1982: 44, 51, is to be added.

[118] Similarities with the Tell Halaf reliefs led Herrmann to tie the 'flame and frond' ivory school, of which the Nimrud pyxis forms part, to a production centre at the former site. On her rationale for the 1000–850 BCE Halaf dating see Herrmann 1989: 101, 107 and 1992: 68, raised later—1996b: 159 and 2000: 276—to the eleventh–ninth centuries. This quarter-millennium range, incomparably long, and ultra-high beginning, reflect in the first place Herrmann's accord with those who dated Kapara to the early or mid tenth century. The 'traditional' 1000 BCE dating of Ahiram played a role too (Herrmann 1989: 105). See further note 120.

[119] On the differing opinions concerning the production centre of the 'flame and frond' ivories cf. Herrmann 2000: 275–276.

[120] On the re-emergence of royal West Semitic art no earlier than the ninth century cf. pp. 61–62. On the other hand, even though very little is known about it (e.g., Winter 1983: 183–184, and *passim*), the existence of non-monumental art (ivories, bronzes, etc.) in Luwian centres in northern Syria in the tenth century is as good as certain. Yet booty, including the north Syrian ivories, arrived in Nimrud chiefly in the ninth–eighth centuries. And so, even if the ivories included tenth-century pre-Aramaean, i.e., exclusively Luwian, items, these must have been a minority that has thus far remained unidentified.

[121] Discernible discomfort was caused by the said similarities to authors who considered Ahiram and the ivories to be of different periods: Gubel (1996: 143), who dated Ahiram some 400 years earlier than the Well AJ pyxis, was compelled to dismiss the similarities as a "deviation". In placing the Ahiram banquet before the ninth century, Bonatz (2000a: 175–176, and *passim*; 2000c: 88–89) was merely following everyone else. And he certainly had difficulty fitting the sarcophagus scene, dated either *ca.* 1200 or *ca.* 1000 BCE (decision evaded) into his sequence. But see Bonatz 2000a: 62–63 and 2001a: 169, where the early tenth century is preferred.

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2. Banquet scenes on funerary steles

Comparable to Ahiram's banquet in their monumentality and funerary role are scenes on Syro–Anatolian steles, considered by Bonatz (2000a; 2000b) an Iron Age innovation. Closest to the sarcophagus scene is Stele Type 3a—a man or woman seated at a table, faced by a standing man waving a fly-whisk (Bonatz 2000a: 38–40). And whereas Bonatz's faith in the high dating of Ahiram remained unshaken (see note 121), he assigned this stele type for the most part to the eighth century (Bonatz 2000a: 59). Gerlach (2000: 253), approaching the subject from a different angle, was for an even lower dating of the said monuments—the eighth–seventh centuries. I should add that although such a late setting supports my effort to down-date the Ahiram scene and the Byblos inscriptions in general from the tenth century, I would not be surprised if a re-examination elicits well developed examples of Stele Type 3a already in the ninth century.[122]

3. Not Late Bronze Age

Banquet scenes are well represented in Late (and Middle) Bronze Age art, but I consider it a mistake to resort to them in seeking to establish the date of Ahiram's banquet (see above). That the Ahiram reliefs have earlier comparisons is only to be expected considering the second-millennium roots of Iron II art;[123] but the execution of several key details in the sarcophagus is distinctly Iron II, as noted. What is more, influence from Egyptian Third Intermediate Period art—which in turn harks back to the New Kingdom—in Ahiram's mourning women (Fig. 15), for instance, or their garments, could be misinterpreted as Late Bronze, if not simply New Kingdom (see Winter 1992: 140, about a related phenomenon on ivories). All these are of no consequence, however, in light of Ahiram's decisive Iron II innovations listed above. In other words, taking the Late Bronze roots of Ahiram's Iron II imagery as evidence of a thirteenth-century origin would be like dating an archaeological assemblage by an obvious collection of heirlooms.

4. Ahiram's archaeological context—or lack of it

In addition to palaeographical and art-historical considerations, attempts were made at times to date Ahiram by finds purported to have come from the burial chamber (chiefly alabaster fragments with the name of Ramesses II) or the fill of the shaft, and these attempts continue to be cited (e.g., Benoit 2003: 341). Unfortunately the

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[122] Bonatz's dating (2000a: 61) of the same scene on ivories, including the pyxis from Well AJ at Nimrud, exclusively to the second half of the eighth century (up to 400 years later than Herrmann!) is probably also too low—compare note 118. Incidentally this low dating was also held by Muyldermans (1989: 393–394).

[123] On the archaeological context of the sarcophagus see next point in main text.

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tomb, accessible via an underground passage from another tomb, was hopelessly robbed, and its small finds were out of context. Besides, the credibility of the Byblos excavation, recording and publication rules out all dating endeavours by context anyway, including attempts to divine the age of Ahiram by the latest sherds claimed to have come from the fill of the shaft. Only a few, like Spiegelberg (1926) and Lidzbarski (1927), saw from the beginning through the Byblos excavators' 'contextual' allegations, and thus held that the sarcophagus must be dated based solely on its inscription (and, I would add, reliefs)—to the early first millennium. Indeed, the sarcophagus can be dated on its own merit only.

5. AHIRAM AND THE BYBLIAN 'PSEUDO-HIEROGLYPHIC' SCRIPT

A phenomenon invoked from time to time (e.g., Sader 1998) is the traces of pseudo-hieroglyphic inscriptions Martin (1961) said he saw under most Phoenician inscriptions from Byblos, including Ahiram's sarcophagus. I am all for this idea, which could substantiate the overlap I propose for the 'pseudo-hieroglyphic' and alphabetic scripts at Byblos in the early ninth century (p. 54). Yet these underlying 'pseudo-hieroglyphic' texts may well be a figment of Martin's imagination: Not one of those who quoted Martin ever professed to have seen them with their own eyes; nor was I able to make out these texts on the originals or their photographs (see note 15).

6. A MIDDLE BRONZE AGE SARCOPHAGUS?

Since the other eight tombs in the royal cemetery are of the Middle Bronze Age, an identical origin of the Ahiram sarcophagus is not to be rejected *a priori.* Also thus dated, the eventual receptacle for Ahiram's mortal remains would be unique at Byblos, where the other early-second-millennium royal sarcophagi are plain. The possibility of a Middle Bronze origin was raised by Jean Délivré, a conservator–restorer of sculpture and former stone mason (Délivré 1998).[124] Even if Délivré's argumentation did not convince me in all its details, his main conclusion, that what we have before us "may be a complete re-carving of a massive Middle Bronze Age sarcophagus, [executed] where it has been standing for many centuries" is to be reckoned with. The re-carving would pertain to the reliefs; the lions on which the receptacle rests must have been part of the original sarcophagus whatever its date.

These lions, crouching and partly three-dimensional, have Iron II as well as second-millennium parallels. Porada (1973: 363), to come back to her briefly, had to make do with architectural "Neo-Hittite reliefs of the ninth–seventh centuries

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[124] This option seems likely to have been suggested before, though I know of no such previous mention.

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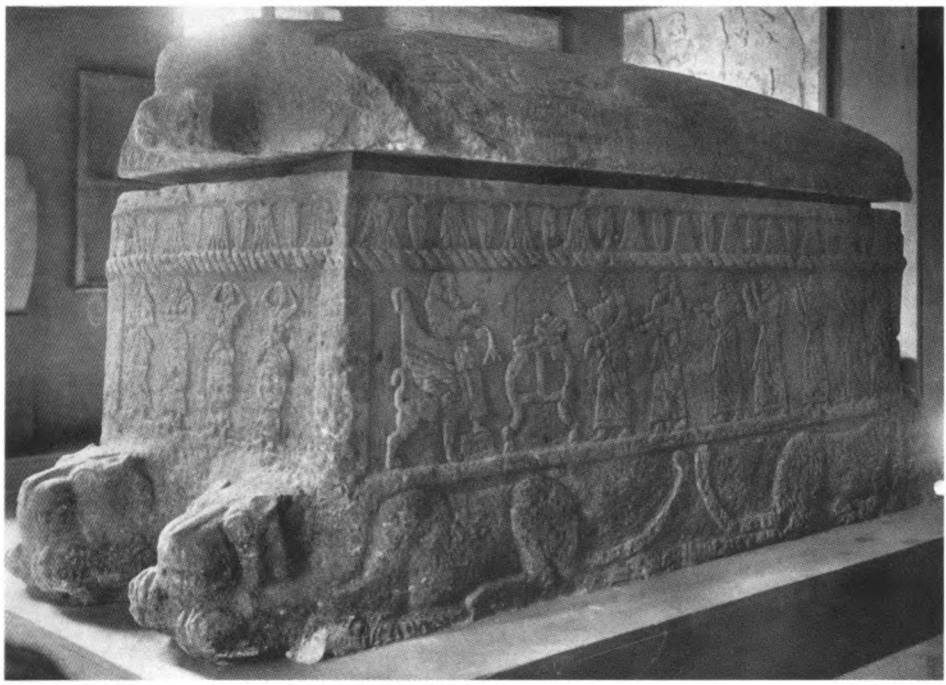

Fig. 15. Byblos, Ahiram sarcophagus.

B.C." showing deities on lions, for she found no closer comparisons; nor can I point out other early-first-millennium receptacle-supporting lions in monumental art. The Iron II existence of such lions is made certain, however, by the evidence of two sources combined: (1) In the ninth-century Taanach stands (Fig. 16)[125] the lions have a three-dimensional front and two-dimensional side like Ahiram's, even though the Taanach felines are not monumental and are standing rather than crouching. (2) Monumental, crouching lions support the Tell Tayinat column bases (Orthmann 1971: pl. 52d).

Yet if a second-millennium parallel is sought,[126] I would add that crouching lions, likewise supporting a monumental receptacle, can be seen on a cult basin from Temple B1 at Ebla discovered prior to the excavations (Matthiae 1965: 71–73; Matthiae, Pinnock and Scandone Matthiae 1995: 422; Fig. 17 herein). Also found in Ebla were two bases thought to have held standing royal statues, each base with two addorsed pairs of lions as on our sarcophagus. One was unearthed in the 1970s in the western palace (Matthiae 2000), the other in 1997 in the 'Place des cisternes'

[125] Tenth century in the earlier literature (compare Lapp 1969: 42–44 with Finkelstein 1998). The Taanach–Ahiram comparison was pointed out by Weippert (1992: note 118).

[126] When the sarcophagus in its original state was either plain (save the lions underneath) or decorated with now-erased reliefs.

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Fig. 16. Taanach, Lapp's cult stand.

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Fig. 17. Ebla basin.

(Matthiae 1998: 568–571; Fig. 18 herein).[127] Even though the Ebla monuments differ from Ahiram's sarcophagus in several aspects, the chronological comparison may gain in plausibility in light of both the funerary character of Temple B1 (Matthiae 1979; 1981) and Délivré's bold proposal. Furthermore, the reliefs decorating the Ebla basin include a scene of the king's (funerary?) banquet.

And if Middle Bronze as well as Iron II comparisons to the lions supporting the Byblos sarcophagus are present, a Late Bronze parallel, the colossal limestone receptacle from Boğazköy (Bittel 1974: 65–68; Opfermann 1993) comes as no surprise. Ahiram's reliefs, in either case, were executed in Iron II, as noted.

Conclusion

The impossibilities to which many authors were driven as a result of maintaining the high dating for Ahiram (whether 1300/1200 or 1000 BCE) even after the publication

[127] The western palace is located in Area Q of the lower city, the 'Place des cisternes' between Temple P2 and Monument P3 of the Ishtar sacred area. The bases were found without their statues. With the base from the 'Place des cisternes' another base was discovered, smaller and comprising only two addorsed lions. The mortises are the same dimensions, and a standing statue, a fragment of the lower part of which was uncovered nearby with its tenon of suitable dimensions, may have originally fit into one of them (Matthiae 1998: 568). A second lower fragment of a standing statue was found in 1993 in the neighbourhood of Temple D (cf. Matthiae 2000: note 12).

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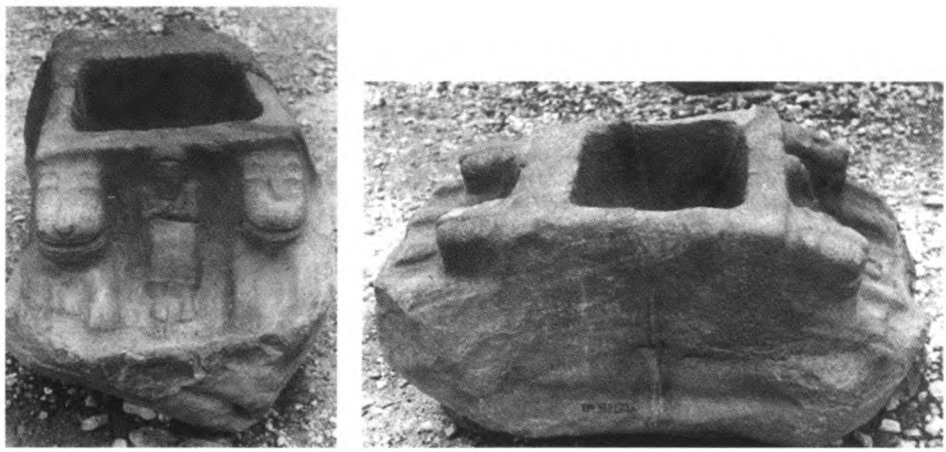

Fig. 18. Ebla statue base.

16 years ago of the Nimrud pyxis (see note 121) underline once again the formidable staying power of the 'traditional' chronology. By proposing to assign Ahiram's reliefs *and* inscription to a date after the tenth century I hope to have put a working hypothesis in place which suits the archaeological and epigraphical evidence better. This, in turn, is founded on Iron II innovations in the reliefs that make all seeming second-millennium features Late Bronze survivors in Iron II. At the same time I am aware that the above is far from doing full justice to Ahiram's sarcophagus: I deal only with the banquet and the lions, save a short reference to the mourners.[128] (On the cup held in the king's hand see p. 38.) Nor did I develop the point that the North Syrian comparisons make the reliefs appear 'less Phoenician' than the provenance would have one expect. The uniqueness of the sarcophagus in Iron II (and probably second-millennium) Phoenicia, and in the entire Levant, cannot be overemphasized.[129]

[128] Whereas I did not verify that the other reliefs on the sarcophagus belong to the ninth–eighth centuries as well, the possibility that some of them date to the Late Bronze Age is unlikely: the banquet scene does not appear to be a later addition to the other reliefs.

[129] Ellen Rehm of Frankfurt has written a monograph on the sarcophagus (Rehm in press), and kindly sent me her chronological conclusions in advance. They differ from mine.

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EXCURSUS B. NON-HEBREW INSCRIPTIONS OF THE NINTH CENTURY

Most of the inscriptions whose ductus is the next stage after the archaizing Byblos script are listed below, divided into five groups. The texts in the first two groups are all from the second half if not last third of the ninth century. In other words, *no historically-dated texts in the linear alphabet are recorded from before 850/830 BCE*. "Undated" in Groups 4 and 5 refers to criteria other than palaeographic.

1. Historically dated monumental inscriptions

The two Mesha texts and the Dan, Fekheryeh and Kulamuwa texts. (The historically-dated Zakkur stele [*KAI* 202] and Arslan Tash gate lions [provisionally Röllig 2000: 182–183; Hawkins 2000: pl. 105] may be slightly later—from the early eighth century.)

2. Historically dated non-monumental inscriptions

The Hazael bronzes and ivories (Fitzmyer and Kaufman 1992: 12–13, 22–23) and Kulamuwa gold ferrule (*ibid.*: 15; Tropper 1993: 50–53).

3. Archaeologically dated non-monumental inscriptions

The Hazor sherds (pp. 85–86), Dan and En Gev sherds (Fitzmyer and Kaufman 1992: 11–12), Tel Kinnerot sherd (*HAE* I: 65, *HAE* III: pl. IV: 5—probably Aramaic), H. Rosh Zayit sherd (pp. 38–39), one or two of the Tel Rehov sherds (Mazar 2003). (On non-monumental Hebrew inscriptions dated archaeologically see pp. 87–88.[130])

4. Undated monumental inscriptions with similar script

The archaic Cyprus inscription (*KAI* 30), Nora stone (Zuckerman 1991), Nora fragment,[131] Bosa fragment (Röllig 1995: 198–199) and Amman Citadel inscription

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[130] Yet another inscribed object, the Revadim seal, has a non-palaeographical means of dating to the early first millennium (its iconography), and despite this, attempts have been made to 'age' its inscription, so that it could be presented as Proto-Canaanite of the twelfth century (references in Sass 1988: 93–95). I classified the seal first (1983) as early Phoenician of the ninth century, yet in 1997 (*WSS* 1067) preferred "Philistine(?), ninth century" (already hinted at in 1985: 112, 225 = 1988: 95, 160), whereas Lemaire (2000a: 246) was for what he termed a Philistian origin and an eighth- or seventh-century date. I now assign the seal to the ninth or eighth century by clear Palestinian glyptic parallels to the engraved scene (parallels which, employing the 'traditional' chronology, I previously considered to belong to the tenth–ninth centuries), as well as by the earliest seals with alphabetic inscriptions. The seventh century, the lower end of Lemaire's range, seems to me for palaeographical and iconographical reasons too late even against the background of our still imperfect acquaintance with the Philistine (or Philistian) script.

[131] Its script, from about the ninth, not eleventh century (Sass 1988: 91–93, with earlier literature; Amadasi Guzzo 1995a: 23–24) is close to that of the Nora stone.

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(*CAI* 59). The attribution and dating of the badly-preserved Ördek Burnu stele (Lemaire 2001a: 187) remain unresolved.

5. Undated non-monumental inscriptions with similar script.
The Tell Halaf 'altar' (note 47), Gezer calendar,[132] and two Byblos inscriptions—cone B and the Abdô sherd.[133]

Some of the inscriptions in the last three groups were occasionally pinpointed to the first half or middle of the ninth century or, following the 'traditional' chronology, placed in the tenth century, all, I think, without sufficient grounds. Quite the contrary—some of the items in Group 3 may by their stratigraphic position belong closer to 800 BCE or somewhat later. The Dan and En Gev sherds, for instance, were probably dated too high by their excavators; likewise most Hazor sherds (cf. pp. 85–87). For Group 4 the second half of the ninth century (or even the early eighth) is perhaps the best guess.

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[132] On the non-Hebrew script and language I agree with Lemaire (2000a: 247) and with others before him.

[133] The latter two and the spatula are mentioned in note 10; on the Abdô sherd see also pp. 31–32. The script of the spatula is more archaic, or archaizing—it is quite close to that of the royal Byblos inscriptions.

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EXCURSUS C. NINTH-CENTURY HAZOR AND THE EARLIEST HEBREW INSCRIPTIONS

No inscriptions were found in the earliest Iron II level—Stratum X—at Hazor. From Strata IX and VIII, both seemingly destroyed in the second half of the ninth century (see p. 87), come five fragmentary alphabetic inscriptions incised on potsherds. Four of them are non-Hebrew, i.e., either Phoenician or Aramaic; a fifth is unclassifiable.

I have further remarks only on no. 3. All commentators read the three initial signs *bet-taw-zayin*, noting the similarity of the first to the Šipiṭbaal and Abdô *bet*. The fourth sign, a vertical stroke, was variously understood as a word divider, punctuation mark or numeral, and the last sign as a ligature of *he* with either another letter, or with a numeral. Be that as it may, the non-Hebrew character of the inscription is underlined by the +-shaped rather than x-shaped *taw*, perhaps also by the uncommon *bet* with its Byblos parallels (if indeed it is a *bet*).

The shape of this *bet*(?) is indeed eye-catching, and I believe no one doubted its identity with the Šipiṭbaal *bet*. Yet two additional explanations may exist, namely that this is either an unsuccessful *yod*, or that it, too, like the fifth sign, is a ligature, this time of a *dalet* (or *ʿayin*?) and another sign. All three alternatives are equally unverifiable; their implications, however, are unequal: A Šipiṭbaal-like *bet* would

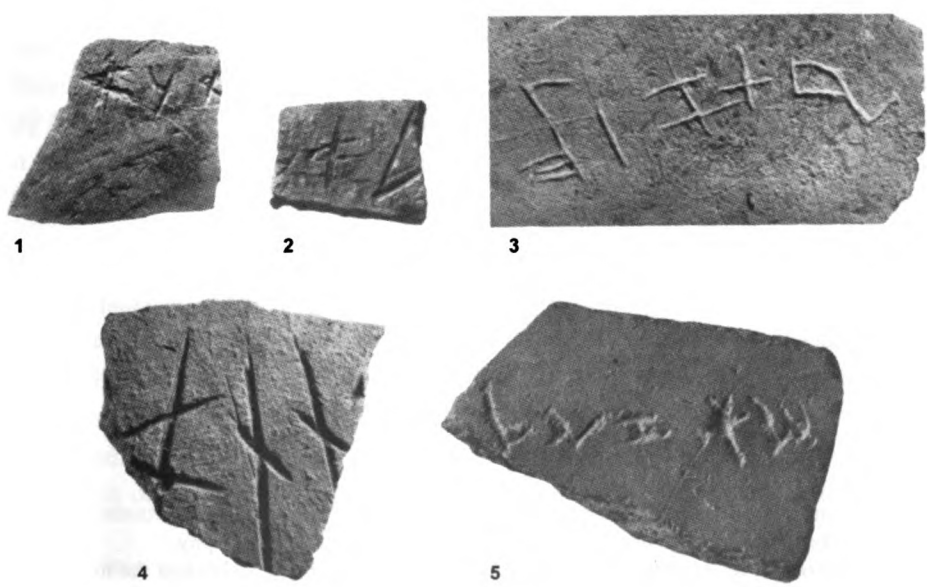

Fig. 19. Five inscribed sherds from Hazor.

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TABLE 5. FIVE INSCRIBED SHERDS FROM HAZOR

No.	*Siglum*[134]	*Stratum*	*Text*	*Attribution*
1	*Hazor* II: no. 1	VIII	...אוא...	Phoenician or Aramaic (*waw*)
2	*Hazor* II: no. 2	VIII	...לאי...	Unclassifiable
3	*Hazor* II: no. 3	VIII	See p. 85	Phoenician(?) (*bet*? *taw*?)
4	*Hazor* II: no. 4	VIII	...תתא...	Phoenician or Aramaic (*taw*s)
5	*Hazor* III–IV: no. 1	IX	...שאזיל...	Probably Aramaic (language)

constitute stratified evidence supporting a late-ninth-century date for some of the royal Byblos inscriptions (see pp. 31–32).[135] Embracing either of the two other possibilities would annul this evidence.

Regarding the significance of the early Hazor inscriptions as an ensemble, if a much larger sample similar in nature had been found, it might have constituted evidence for the non-Israelite character of Strata IX and VIII. (Unquestionable Hebrew inscriptions were found in Hazor in eighth-century levels only.) With our mere five short, fragmentary texts it would be a mistake to claim this; it would likewise be a mistake to claim that Hazor IX–VIII *was* Israelite. On the matching picture at Dan see pp. 84, 87.

Yet even though it already discarded Yadin's 'Hazor, Megiddo and Gezer' dictum, present archaeological research mostly continues to take the 'Israeliteness' of all Iron II strata at Hazor for granted. Differently Finkelstein 1999: 59: "[A]part from the problematic verse on the building activities of Solomon, it [Hazor] is not mentioned as an Israelite city until the eighth century BCE." Compare Kuan 2001: 143–144 (who was probably not yet aware of Finkelstein's remark):

> No other Biblical or non-Biblical inscriptional materials...indicate that the Omrides controlled territory north of Jezreel, making it uncertain whether the Omrides reclaimed any of Upper Galilee. Nevertheless, it has been argued that archaeological evidence from Hazor and Dan...suggests that Upper Galilee—and Hazor in particular—had reverted to Israelite control.

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[134] For *Hazor* II: pls. 169–170, see Naveh 1970a: note 6, and Delavault and Lemaire 1979: nos. 8–11. For *Hazor* III–IV: pl. 357, see Naveh 1989: no. 2, with bibliography.

[135] Yadin (*Hazor* II: 71 with note 6) remarked that this Hazor sherd clinched the higher of B. Mazar's 1946 alternatives for Šipiṭbaal (see note 58 herein). In Yadin's view this indeed constituted a corroboration, for he believed Hazor Stratum VIII to date to the first half of the ninth century (on the later dating employed here see p. 87).

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In a similar vein I might add that the preserved portion of the Dan stele, as is well known, does not locate at Dan itself or in its vicinity the Aramaean victory over Israel. There is more than one potential answer to the question why this stele was erected specifically at Dan.

Finkelstein (1999: 61, 65) attributed Hazor Stratum IX to the Omrides and Stratum VIII to Hazael. I agree with the dating; the political status seems to me a still open question. A fresh study of the ethnic and political situation in the eastern Galilee and northern Jordan Valley in the ninth century, with Hazor and Dan its starting point, seeking to distinguish in Strata X–VII phases of an Israelite, an Aramaean and/or an independent city, is a desideratum.[136] The comparison with Tel Dan is revealing: Prior to the discovery of the Aramaic stele, almost everyone considered all Iron II phases at that site Israelite.

I come now to the earliest Hebrew inscriptions in general. Several sites in Israel—none north of the Jezreel Valley—and possibly in Judah, yielded brief texts, mostly on potsherds, in early Iron II strata (ninth century): Tel Amal, Tel Rehov, Tel Batash, Eshtemoa, and Arad Stratum XI (cf. *HAE* I: 29–39; *Timnah* II: 190–191; Mazar 2003). Remarks to specific inscriptions follow.

Tel Rehov: One or two of the three inscriptions may be non-Hebrew.

Beth Shemesh: An unstratified two-letter Hebrew text (Bunimovitz and Lederman 1997: 48) has been attributed to the tenth century ('traditional' chronology) solely via letter typology. I believe such a date is far too high.

Eshtemoa: For the ink inscriptions on jugs from this Judahite site a dating *ca.* 800 BCE (rather than the tenth–ninth century on the one hand and the late eighth on the other), founded on the combined typology of the letters, the jugs, and the jewellery discovered inside the jugs, was proposed in Sass 2002b: note 11.

Arad: Stratum XI, to which the earliest Hebrew inscriptions were attributed, was dated in the second half of the ninth and first half of the eighth centuries (Herzog 2002: 14; Singer-Avitz 2002: 116). See also Na'aman's reservations (2002: 22–23).

Non-Hebrew inscriptions occasionally identified as Hebrew: On the H. Rosh Zayit sherd see pp. 38–39. On the Gezer calendar see p. 84.

Personal seals inscribed in Hebrew (among other West Semitic scripts) are documented with certainty from the eighth century onwards; they may have begun in the ninth (cf. note 130), but would be out of place earlier. Monumental Hebrew inscriptions of the ninth century are still altogether missing (the earliest date to the

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[136] Combining the inscriptional and biblical evidence with a synthesis of the settlement pattern and material culture in its entirety, such a study will certainly prove a rewarding subject for a doctoral dissertation.

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eighth century), yet they certainly existed: The concept of monumentality—as exemplified by the late-ninth-century Mesha stele—would have been learned by the Moabites across the River Jordan along with the script, which at this early stage in Moab was practically identical to Hebrew (Naveh 1982: 65–66).

Concerning literacy among the Hebrews, the following picture emerges from the above data: Despite lagging behind Israel in social and political development (Jamieson-Drake 1991; Finkelstein 2001), Judah is unlikely to have remained oblivious to the Hebrew branch of script once this branch emerged in the ninth century in the northern kingdom (pp. 59–60). While even the best archaeological picture available—at Arad—is indecisive, I believe that clear-cut archaeological evidence for the Hebrew script in Judah as early as the ninth century will one day be discovered.

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EXCURSUS D. DATING THE AIN DARA SCULPTURE AGAINST ITS SYRO–HITTITE BACKGROUND

The pre-ninth-century cultural and political position of the Aramaeans, prior to the emergence of royal cities and alphabetic writing, is outlined in Section 1.4.2.1 (pp. 61–64). The pertinent Assyrian documentation is referred to there, and Assyria itself in the same period is also briefly addressed. The contemporary Luwian sources (with the possible exception of those from Til Barsip—p. 57) do not deal with the Aramaeans directly, and were, therefore, not included in that section. Nevertheless, the Luwian circumstances before the ninth century are of relevance to the understanding of the Aramaeans. They are therefore delineated here.

It is conceivable that when Hattuša's backing came to an end, Carchemish soon lost control of most of its former vast viceroyalty. With the fall of the Syrian vassal kingdoms of Ugarit, Amurru, etc., and with urban life in general gone, too, it is possible that—after a gap of variable length from one region to another[137]—principalities and city states began to fill the void. Many of these local powers—in the first 250 years practically all of them—wrote in Luwian. The large area where Iron Age Luwian inscriptions are found can be divided into a mixed Luwian–Aramaean south and predominantly Luwian (at any rate un-Aramaean) north.[138]

Very probably, this recovery began first in parts of the northeast, where the urban fabric may have been less affected by the upheavals that marked the end of the Late Bronze Age. Yet even for this region, the Luwian documentation for ruling dynasties and their royal cities of the twelfth–eleventh centuries is confined almost entirely to the continuity in Carchemish itself and, beginning soon afterwards, in Melid and its land (Hawkins 1995: *passim*).

As far as the mixed Luwian–Aramaean south before the late tenth century is concerned, the evolution and interaction of the two groups still pose a largely open question (compare note 80). The diverse historical reconstructions consist mostly

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[137] See for instance Hawkins 2002: 148: "We may infer from the scanty indications that with the elimination…of Hattusa, Karkamiš, the effective controller of Syria, was left holding the line of the Euphrates from Malatya to Meskene, and claimed the vacant title [of Great King]. Subsequently and probably quite rapidly, even this rump was dissolved into the independent kingdoms of Malatya, Kummuh and Karkamiš, to the west of which Gurgum and Unqi were also formed." For a slightly different view of Hawkins on Unqi/Patin see note 143 herein.

[138] 'North': Carchemish, Melid, Gurgum, Kummuh, Que, Tabal. 'South': Sam'al, Patin, Arpad and Hamath (the political status of Aleppo before the ninth century remains unclear; on Til Barsip see p. 57). At Sam'al no monumental Luwian inscriptions were found, but some of its rulers bore Luwian names. In addition to the north and south a third region, the Aramaean (and un-Luwian) 'far south', is mentioned on p. 59 in relation to Aramaic writing.

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of informed conjecture and only very little substance:[139] The Luwian inscriptions known thus far do not begin here before the ninth century. Anepigraphic monuments, that Orthmann (and others) dated by their style to the tenth century and earlier, come from Sam'al, Ain Dara and Hamath,[140] yet the problems inherent in Neo-Hittite datings based solely on art-historical considerations are well known (see below). All the same, a tenth-century Luwian beginning in the 'south' does not seem out of the question. Not so twelfth–eleventh-century beginnings: If Luwian rulers based in royal cities rose in the south beyond Carchemish by the twelfth or eleventh centuries, they are as yet undocumented. In other words, most Iron Age Luwian inscriptions anywhere are from the ninth and eighth centuries (cf. Hawkins 2002: 147–148).

The Assyrian documentation concerning the 'south' (Assyrian 'west') points much in the same direction: "After the encounter of Ini-Tešub [of Carchemish] by Tiglath-pileser I in *c.* 1100 B.C., there are no references to any states until Aššurnaṣirpal II undertook a similar campaign *c.* 870 B.C." (Hawkins 2000: 75). In the same documentation the Ahlamu–Aramaeans, pre states and royal cities, are attested several times (pp. 63–65).

Indeed, what was the status at the time of places in northwestern and central Syria such as Tell Tayinat, Ain Dara, Aleppo and Hama? Faced with the extant dearth of data I would opt for a minimalist picture:[141] It is possible that, unlike further north, the circumstances in much of northern Syria were not very different from what they were in much of the Iron I southern Levant, where the urban recovery began in the tenth century.

At least on Ain Dara some readers may object. Whereas it is true that inscriptional and stratigraphic evidence does not exist (cf. Stone and Zimansky 1999: 3), most authors argued that, judging by the style of the monuments, the twelfth century is represented.[142] Yet the Ain Dara temple as a twelfth- or eleventh-century structure, according to what we know, would be a striking exception so far south and west. On the one hand, the elaborate Neo-Hittite decor of the upper-city temple could hardly have come to be without an established power behind it. On the other, who would

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139 Similarly Aro 2003: 294: "For a general reader with no expertise in Anatolian and north Syrian archaeology, different datings given in different handbooks, often written by very prominent scholars, can be somewhat confusing."

140 References in Hawkins 1995: 95; 2000: 402. The reliefs uncovered in the late 1990s in Aleppo probably belong around 900 BCE—cf. Kohlmeyer 2000a; 2000b; Orthmann 2002: *passim*.

141 For a maximalist approach see for instance Mazzoni 1995: note 45; 2000a: 31–34; a more reserved view was expressed by Hawkins (1995: 95).

142 All the same opinions differ widely on how much the date extends on either side of the twelfth century—from a maximum total of more than half a millennium, 1300–740 BCE (Abu Assaf 1990: 39–41), via 300 or 200 years, 1300/1200–1000 (Orthmann 1993: 248–251), to a minimum of a single century, 1150–1050 (Mazzoni 2000b: 1043–1048). One is reminded of the range of stylistic datings proposed for Ahiram's reliefs (pp. 75–77).

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have ruled Ain Dara then? Far-away Carchemish? An early, unattested kingdom of Aleppo or Mukiš/Patin? An equally unattested local king?[143] Attempts to counter these doubts with stylistic arguments alone may seem somewhat strained.[144]

I have always been rather incredulous of the confident art-historical dating, founded largely on the presupposition of a simultaneous, linear development of much of the Syro–Hittite sculptural style, with so little support from textual and contextual evidence.[145] Should not the conclusions founded on such an axiom at least be reviewed periodically? Could not differences in style transpire on reflection as other than chronological (geographical, cultural…)? Might not stylistic similarities, in turn, denote in some works of art merely a predominance of long-lived traits, or else betray a deliberate archaism?

The sea change has at last arrived, admirably carried by Winfried Orthmann himself (2002: 153): "The problem of dating the diverse groups of monuments is…by no means definitely solved. Both the stylistic development of Late Hittite art and the development of its iconography remain objects of [ongoing] research." On ascribing chronological significance to stylistic differences and similarities Orthmann wrote as follows (2002: 154, 155; italics mine—*B.S.*):[146]

> It should thus be assumed that despite the short distance between them, Hattuša and Alaca Hüyük were characterized by differently nuanced artistic traditions. Such an interpretation facilitates in many respects the understanding of the artistic developments of the following centuries. *It deprives us, however, of the foundation for the relative dating of works of art* not originating from the same place. Whereas we are still able to account for assumed contemporaneity with the establishment of stylistic closeness, *we can no longer deduce a time interval from difference in style*…. If the Arslantepe reliefs are taken to belong to an oldest stage of the Late Hittite art and those from Carchemish to a second stage, then it becomes difficult to accommodate the Aleppo reliefs, at any rate when a uniform development of Late Hittite art is presupposed.

Uniformity in Late Hittite art is hardly to be expected. This was expressed in a most common-sensible manner by Aro (2003: 293): "[B]ecause the Luwian city-states lacked a centralized power to set the standards for a style, as for example in the Neo-Assyrian empire, a clear homogeneous development in style is not traceable."

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[143] Hawkins (2003: 150) coped with the problem as follows: "The…early Iron Age temple of 'Ain Dara…may antedate the establishment of the Neo-Hittite state [of Patin]…."

[144] If the chronological problem of Ain Dara is cleared up one day, this may bring research closer to the solution of other questions concerning the site: Who built the temple? What can be gleaned from the coincidence of the Levantine plan and 'Hittitizing' decor? What was the political status of Ain Dara at that time? What was its ancient name?

[145] See also Aro 2003: 296–297 on the plethora of stylistic datings for uninscribed monuments or such that cannot otherwise be linked to historical evidence, and on the grave chronological problem posed by reuse.

[146] On the Aleppo reliefs see also note 140 herein.

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I would add that exceptions certainly exist also to the inferring of contemporaneity from similarity in style, even in works of art originating from the same place (Orthmann's second point).[147] Such similarities, as noted above, could in certain monuments be the result of a deliberate archaism. If non-archaizing traits reflecting the sculptor's own day, few though they may be, are sought and indeed detected, with luck, the 'true' date of such works could be revealed.

Against this background two additional possibilities—both positing an early Iron I gap instead of the LB-to-Iron-II continuum hitherto argued by most—should be considered for the Ain Dara artwork:

1. That the temple was constructed under the Empire, then renovated early in the first millennium.[148]

Or,

2. That all phases of the temple date to the early first millennium and are decorated in a mostly archaizing style, perhaps also re-using sculpted blocks from a yet-to-be-identified Empire period structure nearby.[149]

With the available pre-tenth-century Luwian sources so poor, an urban gap in the 'south' (and in much of the 'north'?) in the twelfth–eleventh centuries may be conceivable. The settlement picture of the period, in the mixed Luwian–Aramaean south at any rate, was probably similar to much of the southern Levant, where small, non-urban sites predominated. (See addenda on p. 183.) This is indicated in the Assyrian documentation, and could be borne out by the results of new surveys and excavations in northern Syria (some references in note 97). A recovery had begun in the course of the tenth century. The way was led by Luwian dynasties, whose system of principalities and their royal cities followed the model of Carchemish and Melid.[150] Towards 900 BCE the Aramaeans had matured sufficiently to join this process, which in due course they dominated.

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147 For analogous long-lived traits and archaism in monumental alphabetic inscriptions see Section 1.3.2.2 (pp. 28–31); in monumental art—p. 77.

148 Little is thus far known about Empire reliefs in Syria, but this may change if Özyar (1992 and 1998) was right on the widespread reuse of second millennium sculpture in first-millennium sites. (Some of Özyar's reasoning for the Carchemish reuse seems to me better-founded than her arguments for a similar phenomenon at Tell Halaf.)

149 A tenth-century founding of the temple was proposed by Hrouda (1991: 372), yet as a corroboration for this dating he cited the biblical description of Solomon's temple. (Of no direct relevance to this excursus are studies such as Monson 2000 or Matthiae 2002 on the Solomonic temple itself, that cite Ain Dara as a parallel.)

150 This could find a parallel in the late Iron I 'New Canaan' revival in the south Levantine lowlands (p. 67).

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EXCURSUS E. ON THE CHRONOLOGY OF GOZAN

This excursus presents in full the inquiry excerpted in the discussion of early Aramaean statehood (pp. 61–63).

The dating of Kapara has fluctuated in the scholarly literature since the Tell Halaf excavations in the early part of the twentieth century (references in Dion 1997: 43). The first half of the ninth century as the time-frame for Hadianu and Kapara was advocated in the excavation report, and in recent years this has won wide (Dion, *loc. cit.*) though not universal acceptance.[151] In 1979, at neighbouring Tell Fekheryeh, the statue of Hadyisʿi, son of Sasnuri, with its Assyrian–Aramaic bilingual inscription, was discovered. Sasnuri and Hadyisʿi were understood to have reigned in or around the 860s and 840s respectively (Abou Assaf, Bordreuil and Millard 1982: 112–113), and this corroborated from an additional perspective the said chronology of Hadianu and Kapara (see below, with notes 152, 153). Yet a true historical anchor for the four kings is lacking; the above datings as well as all the proposed alternatives derive exclusively from the evaluation of indirect evidence.

My evaluation is presented below in tabular form. Four variant chronologies for the two pairs of rulers are fitted around three key dates—896, 855 and 808 BCE. One variant is taken from the comprehensive study of Keller (1997), who in turn followed the Fekheryeh editors on Sasnuri and Hadyisʿi. All the other three possibilities place Hadianu and Kapara a few decades later than Keller's choice; the second possibility reckons with Millard, whereas the third and fourth have lower dates for Sasnuri and Hadyisʿi as well. The considerations for these chronologies are:

- *The 896 BCE mention of Abisalamu.* This ruler of Gozan paid tribute to Adad-nirari II (911–891) in his seventeenth year, during his fifth campaign against Hanigalbat. The first possibility regards Abisalamu as a successor of Hadianu and Kapara, and the three other possibilities, which I prefer, regard him as a predecessor: That the odds are against an Aramaean royal city with monumental architecture and art in the proto-historic stage of the tenth century is discussed elsewhere in this book (e.g., p. 63). All the same, a pre Abisalamu date at the very end of the tenth century may just be barely possible for the two monarchs.
- *Millard's suggested 866 BCE eponymate of Sasnuri.* The first and second possibilities are founded on it, and the third and fourth are not. The fluctuations in ninth-century Gozan between an Assyrian province (Sasnuri) and a principality (Hadyisʿi) implied by Millard do not in themselves weaken his suggestion; such was the norm in the region (see p. 62 with note 93). But also if the eponym option, quite plausible yet unverifiable, is abandoned, the historical dating of

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[151] For a late-tenth-century dating see below; on *ca.* 1100 see p. 76; on the late ninth century—Zadok 1995: 232, citing Lipiński; see also Lipiński 2000: 132.

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Hadyisʿi can hardly go beyond the second half of the ninth century (see below).

- *The 855 BCE liquidation of Bit Adini.* This event, which took place in the fourth year of Shalmaneser III, was no doubt a milestone for Gozan also. The notion that Assyrian influence on Jezireh monumental art followed in the wake of its military and political supremacy is of relevance to the chronological issue addressed herein: It is quite possible that Kapara, with his predominantly non-Assyrian art, reigned before the end of Bit Adini,[152] and more than reasonable that Hadyisʿi, with his Assyrianizing statue, reigned afterwards.[153] All four possibilities in the table below follow these assumptions.
- *The 808 BCE campaign against Gozan.* This campaign of Adad-nirari III (810–783) in his third year was evidently directed against a mutinous province. *Ca.* 810 is thus the lowest possibility for the still debated dating of Gozan's end as an independent principality.
- *A range of* ca. *850–810 BCE for Hadyisʿi.* Millard's suggestion that Hadyisʿi reigned some time between 850 and 825 BCE was founded on the identification of Sasnuri with the eponym of 866 BCE. In light of the uncertainty of that anchor, the reign of Hadyisʿi (length unknown) floats within the entire interval between the annexation of Bit Adini in 855 and the Gozan campaign in 808.
- Further, the second, third and fourth possibilities express minor variations in the dates of the four kings by inserting a hypothetical ruler in three different time-slots.

Finally, which of the four possibilities to choose? All of them have Hadianu and Kapara before 855 and Hadyisʿi afterwards, which is adequately borne out by the textual and figured sources. Any further refinement can only be founded on indirect evidence, as stressed at the beginning of this excursus. Regarding the background of

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[152] Thus for instance Winter 1989: 324: "The particular mixture of Neo-Assyrian and North Syrian elements, superimposed upon a local tradition surviving from the second millennium…would be most consistent with a date…when the Habur had already been exposed to a Neo-Assyrian presence (i.e., after the campaigns of Adad-nîrârî II), but before Shalmaneser III conquered Bit Adini in 855 B.C.… The points of contact between the Tell Halaf reliefs and Assyrian art…are many. Nevertheless, the dominant aesthetic is overwhelmingly Neo-Hittite." On the dating of Kapara before Hadyisʿi see *ibid.*: 324–325. The absence of Aramaic inscriptions on the monuments of Hadianu and Kapara likewise favours the seniority of these two rulers—compare on Terqa on p. 56.

[153] See Winter 1989: 325: "[T]he squat proportions, enlarged eyes, and abbreviated rendering of beard- and hair-curls betray the hand of a local stone-carver; and the conscious adoption of Assyrian court dress, shoes, clasped hands, and overall stance suggest a historical period in which Assyria was setting the standard, and providing the models, for major public works of Guzana.… [O]ne would need to posit a phase of conscious emulation of, if not political association with, Assyria."

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the making of the Fekheryeh statue, a period of relative Assyrian weakness seems reasonable. Within the time range of *ca.* 850–810 mentioned above for Hadyisʿi, the final years of Shalmaneser III, 827–824, during the revolt of his son, Aššur-dan-apli (Zadok 1982: 125; differently 1995: 232), or else the reign of Shamshi-Adad V (823–811) could be suitable. With such a dating for Hadyisʿi, the proposed 866 eponymate for Sasnuri becomes somewhat uncomfortable (compare Röllig 2003: 427); adhering to it necessitates according a very long reign to either father or son, or to both. The chronology is less strained if Sasnuri is assumed to have been a post-855 governor under Shalmaneser III, and his son and successor Hadyisʿi a king, who reigned at a time of Assyrian weakness around 825 BCE (third and fourth possibilities).

TABLE 6. FOUR ALTERNATIVE GOZAN CHRONOLOGIES

<table>
<tr><th>First possibility</th><th>Second possibility</th><th>Third possibility</th><th>Fourth possibility</th></tr>
<tr><td>Bahianu (2nd qtr. 10th)</td><td colspan="3" rowspan="2">Bahianu (late 10th century)</td></tr>
<tr><td>Hadianu (3rd qtr. 10th)</td></tr>
<tr><td>Kapara (4th qtr. 10th)</td><td colspan="3">Bahianu (late 10th century)</td></tr>
<tr><td colspan="4">Abisalamu ruler of Gozan in the days of Adad-nirari II (896)*</td></tr>
<tr><td rowspan="2"></td><td>Hadianu (± 890)</td><td>Hadianu (± 890)</td><td>[undocumented ruler]</td></tr>
<tr><td>Kapara (± 875)</td><td>Kapara (± 875)</td><td>Hadianu (± 875)</td></tr>
<tr><td>Sasnuri (2nd qtr. 9th)</td><td>Sasnuri (eponym 866)</td><td>[undocumented ruler]</td><td>Kapara (± 860)</td></tr>
<tr><td colspan="4">Liquidation of Bit Adini by Shalmaneser III (855)*</td></tr>
<tr><td>Hadyisʿi (3rd qtr. 9th)</td><td>Hadyisʿi (± 845)</td><td>Sasnuri (± 845)</td><td>Sasnuri (± 845)</td></tr>
<tr><td></td><td>[undocumented ruler or Assyrian governor]</td><td>Hadyisʿi (± 825)</td><td>Hadyisʿi (± 825)</td></tr>
<tr><td colspan="4">Campaign of Adad-nirari III against the rebellious province Gozan (808)*</td></tr>
</table>

* Event with fixed date.

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CHAPTER 2

THE ANTIQUITY OF THE ARABIAN ALPHABET

This chapter constitutes 'a view from the North', linking Arabian epigraphy and archaeology with West Semitic epigraphy and Near Eastern archaeology and glyptic. The essence of my earlier relative chronology of Levanto–Arabian alphabetic connections (1985, 1991) remains much as it was. But new evidence and perceptions require changes to both the absolute chronology of the adoption of the alphabet in Arabia and to our understanding of the processes underlying the adoption. In 2001 and 2003, writing the following text in the library of the Institut d'études sémitiques in Paris, I was privileged to have the opinions and advice of François Bron, Iwona Gajda and Christian Robin. I trust that this beneficial influence, gratefully acknowledged, is detectable in the following pages, though—admittedly—I went my own way.

2.1 RECENT EXCAVATIONS IN YEMEN AND THEIR INSCRIBED FINDS

Discoveries published after my earlier work (1991a) have enriched our knowledge of the early South Arabian alphabets considerably: New inscriptions from several places, among them the excavations at Yalâ, Raybûn and es-Sawdâ', have come to light.[154] In the latter site monumental inscriptions considered to belong to the eighth century were found, while the former two sites yielded inscribed potsherds from stratified contexts dated even earlier by their excavators. The age of some of these contexts was determined radiometrically, of others by comparisons with Palestinian pottery. The dating of stratified inscriptions by these methods was previously applied only at

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[154] The inscribed sherds from Yalâ mentioned in de Maigret and Robin 1989: 287–289, and from Raybûn in Bauer 1989, came to my attention only after my 1991 book appeared.

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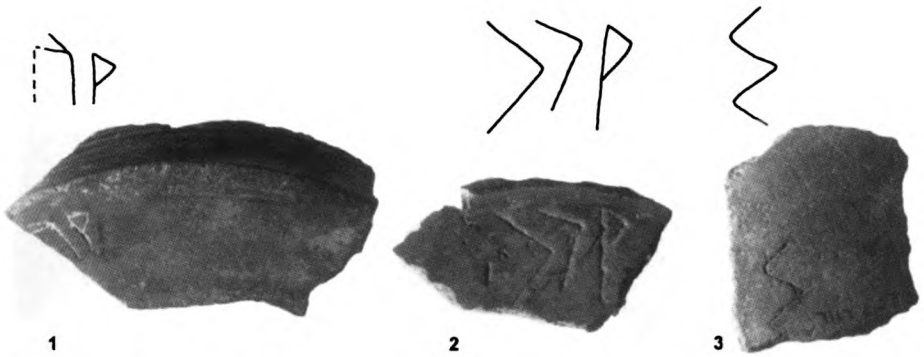

Fig. 20. Yalâ, inscribed sherds from Level B.

Hajar Bin Humayd, excavated more than half a century ago (cf. Sass 1985: 194–195 = 1991a: 33–35). (The 1990s also saw the publication, at least in preliminary form, of a great many cursive, or 'minuscule' texts on palm-stalks that were hardly known before [see pp. 124–128]. These texts surfaced mostly as a result of illicit digging.)

The excavators' views are summarized in Sections 2.1.1–2.1.5; my comments on some of these views appear in Section 2.1.6.

2.1.1 Yalâ

The site is situated southwest of Marib. A building of unhewn stone, 'Maison A', was excavated in 1987. Four levels—A–D—were observed within and underneath it; the two later ones, A and B, yielded written material.

Inscriptions. Four inscribed potsherds from the uppermost Level A and three from Level B were mentioned by de Maigret and Robin (1989: 287–289). The final publication of the inscribed sherds by Garbini (1992) lists the three from Level B and twenty-two from Level A (Figs. 20, 21);[155] some of them consist of a single letter. A decorated and inscribed terracotta plaque (Fig. 22) also came from Level A—Locus 1, which yielded the calibrated radiometric date 850–650 BCE (de Maigret and Robin 1989: 283, 288, pl. 8b; Antonini 1996; not in Garbini 1992).[156] The short text is not clear.

Dating. On the strength of a surface survey the site was considered to have been abandoned *ca.* 600 BCE and not disturbed by later activity (de Maigret 1997: 50). No direct dating by pottery typology of the inscribed sherds was mentioned (though see 'Synthesis' below). The radiometric dates of wooden beams from the site (de Maigret and Robin 1989: 286–290) are as follows:

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[155] Colour photographs of six of the sherds in Bron 1997: 57.

[156] The object may have been inspired by Mesopotamian Lamashtu plaques, chiefly of the eighth–seventh centuries, about which see Wiggermann 2000: especially 219–224.

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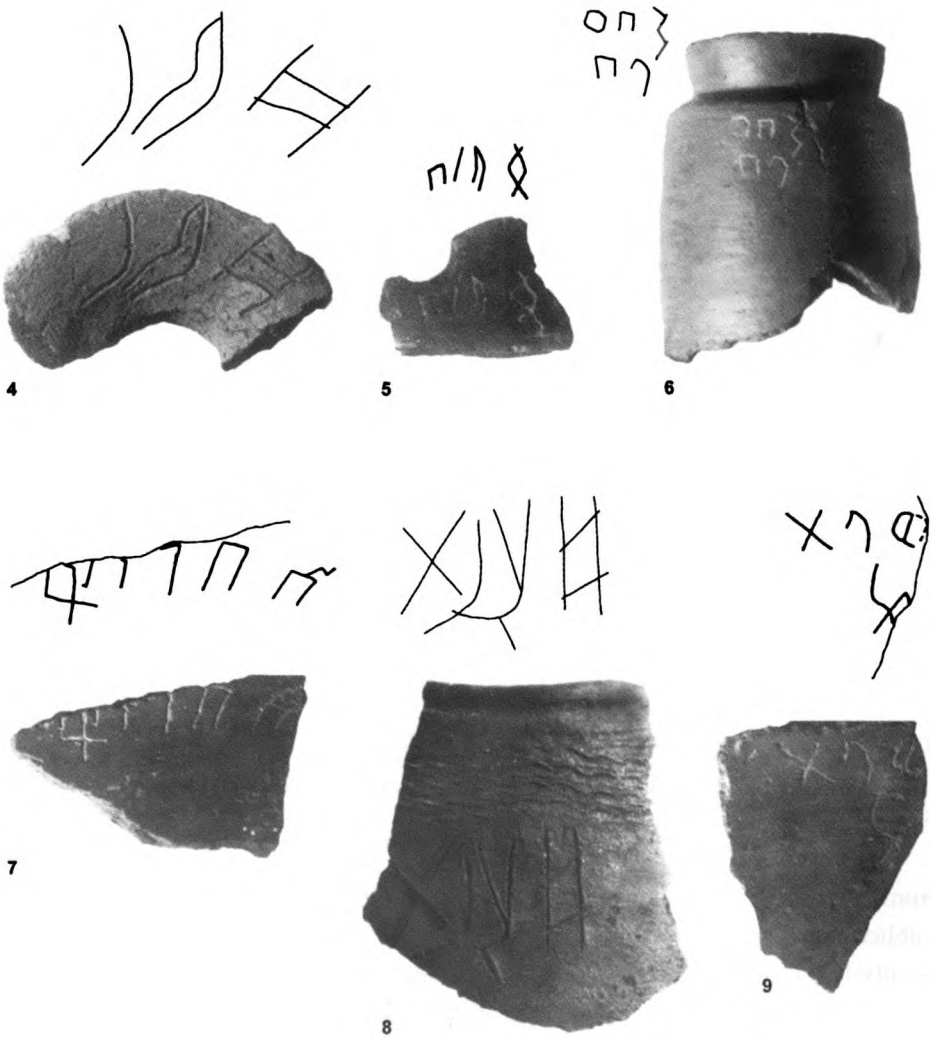

Fig. 21. Yalâ, inscribed sherds from Level A (continued on next page).

Level A. 2600 ± 50 B.P. = 850–650 cal BCE.
Level A. 2570 ± 60 B.P. = 825–585 cal BCE.
Level B. 2750 ± 75 B.P. = 1100–795 cal BCE.
Level B. 2840 ± 70 B.P. = 1240–830 cal BCE.
Level C. 2980 ± 65 B.P. = 1395–920 cal BCE.

These results led de Maigret (1997: 50–51) to date Level A to the eighth–seventh centuries and Levels B and C to the twelfth–ninth centuries. The minimum destruction dates were listed as follows: Level A = 535–600, Level B = 755, Level C

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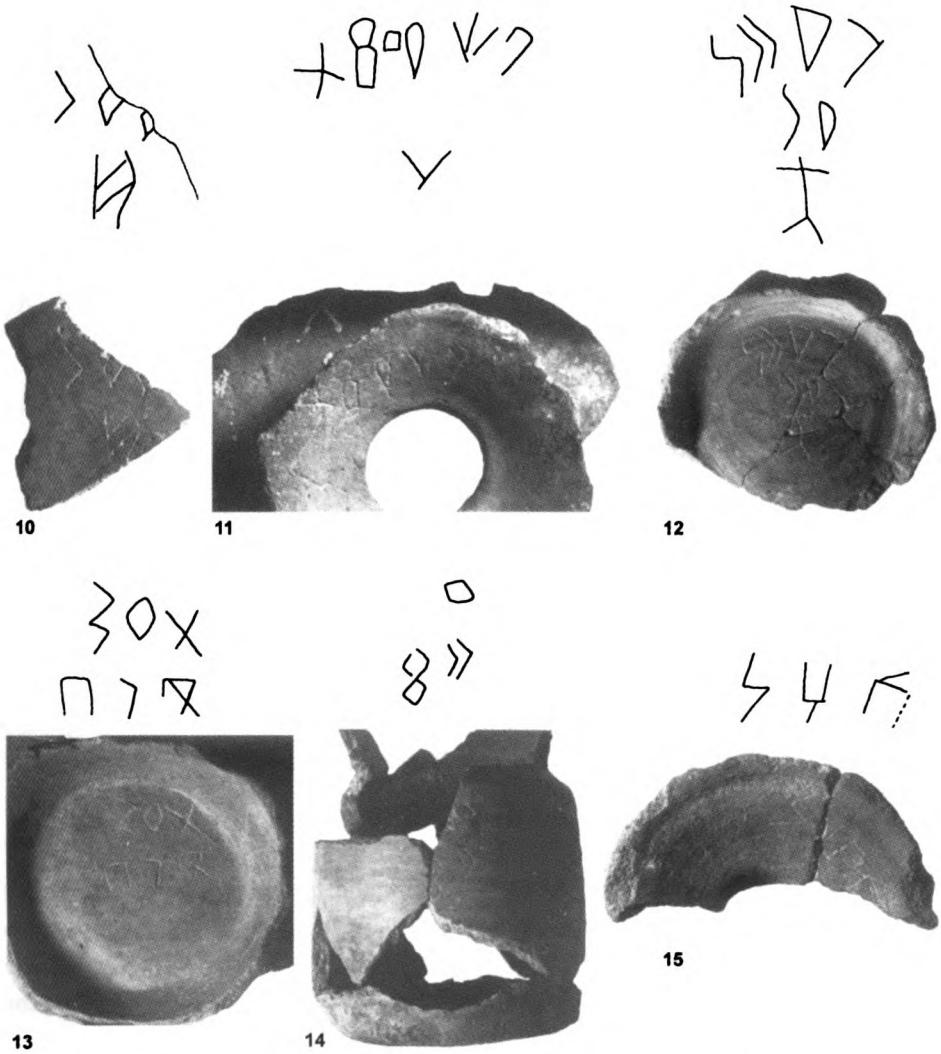

= 870, the *terminus ante* of the Arabian alphabet as 755 (de Maigret 1996: 172–173 = 2002: 183), but this does not seem to have influenced the final conclusions. Earlier (de Maigret and Robin 1989: 288) this view was given a more prominent place.

Synthesis. For the pottery de Maigret (1997: 51) noted on the one hand its similarity to the material from the deep levels N–K of Hajar Bin Humayd, and on the other its difference from the pottery of the oldest levels at Raybûn. It is only towards the end of the eighth century that the Raybûn and Yalâ pottery assemblages become similar, and this assimilation is considered to reflect the political unification under Sabaean hegemony. It was further observed (*loc. cit.*)

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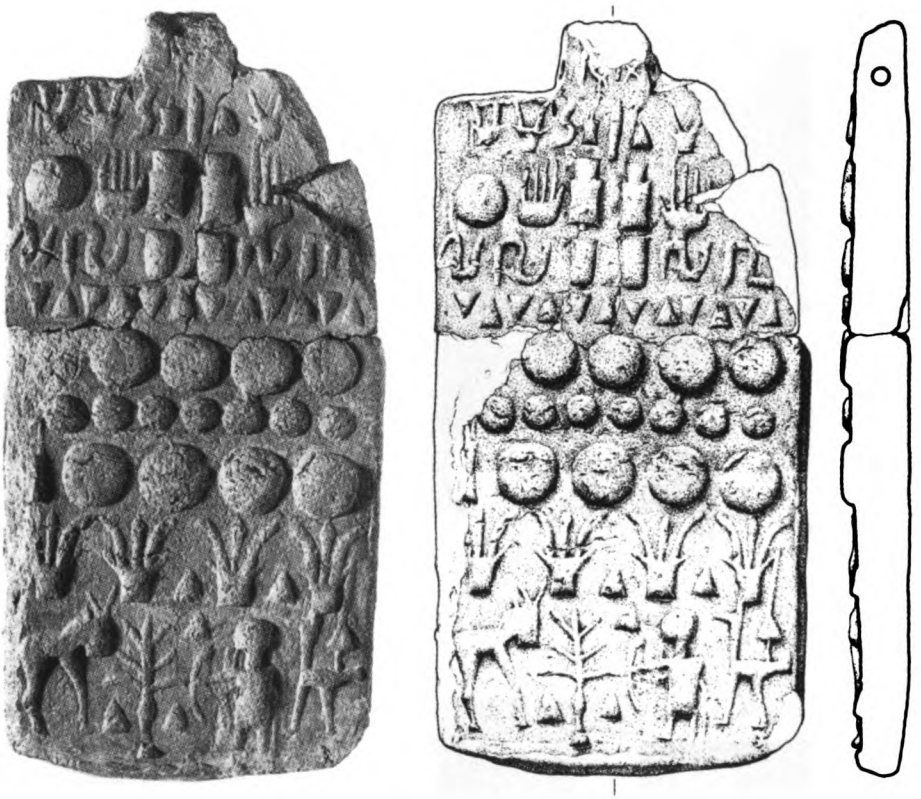

Fig. 22. Yalâ, decorated and inscribed terracotta plaque from Level A.

that the Yalâ material culture shows a remarkable continuity across the three strata.[157] As to the Level B inscriptions, they were thought to have been written "before the tenth or ninth century" (*loc. cit.*) on the strength of the 1240–795 BCE time range indicated by the radiometric analyses. The texts on some of the Yalâ sherds (e.g., nos. 6, 10) were considered by Garbini to be specifically North Arabian personal names.

2.1.2 Raybûn

This site in the Hadramawt was excavated by a Russian expedition between 1983 and 1991. The finds referred to herein come from Area II–III, and from the stratigraphic sounding, Area IV in the southwestern part of the site (Sedov 1997: 43, ill. 3; cf. Frantsouzoff 2001: third, unnumbered plan at the beginning of fascicle B).

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[157] Cf. also de Maigret and Robin 1989: 286: "The pottery found in the diverse strata...does not change as far as typology is concerned, fitting into the usual repertoire of the Sabaean vases."

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Inscriptions. The sounding yielded a number of archaic sherds with two kinds of inscriptions—those integrated into the decor of red paint and those incised on red-slip pottery. As Sedov (1996: 79–80) wrote, the sherds come "from the surface finds and from the upper, probably rather late strata of Area II–III. They might belong to the final stage of the period marked with burnished red slip and red-painted pottery." Illustrations of two painted sherds, specifically referred to as surface finds (Sedov 1997: 57, caption to Pl. 40), with three letters each, *ḥdṯ* and *ʾln*[..., are published (Fig. 23),[158] and a third sherd with a single incised letter is mentioned but not illustrated as far as I know.[159]

Dating. The red painted and slipped pottery of the inscribed sherds belongs to a 'first ware' (Sedov 1997: 44) from the earliest strata—V, IV, and III in Areas II–III and IV.[160] It is distinctly earlier than a second, coarse ware.

For the 'first ware' Sedov (1996: 74–76; 1997: 45) found comparisons in Palestinian Late Bronze Age pottery, referring mostly to Macalister's 'third Semitic period' at Gezer. The similarity was said at one point to pertain not to the shapes of the pots, but only to the painting; a few lines thereafter, however, it was extended to the shapes as well.[161] Parallels to the Hajar Bin Humayd pottery are slight.[162] The said comparisons sufficed (Sedov 1996: 77–79; 1997: 46) to date the Raybûn 'first group' to the last quarter of the second millennium and first quarter of the first millennium BCE. Furthermore, the mix of painted pottery and "numerous...obsidian and flint microlithic tools" (Sedov 1996: 77) was considered another indication of the high initial age of the pottery. Elsewhere this

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158 Piotrovskij and Sedov 1994: Pl. 26: 3; Sedov 1996: Figs. 5: 1 and 6 upper left; 1997: Ills. 38d, 39d, 40a–b; colour photograph of one sherd in Bron 1997: 55.

159 "[S]ome painted sherds have inscriptions in the monumental South Arabian script. In addition, we found several graffiti on the fragments of the burnished red-slip pottery: a single letter *s*[1] incised on the outer surface of a vessel" (Sedov 1996: 79). That the said graffiti equal "isolated incised letters" transpires from Sedov 1997: 46.

160 "The initial settlement was situated at the south-west corner of the site, where we found archaic red-painted and red-slipped pottery, ruins of mud-brick houses and got the earliest ^{14}C dates.... It was found...only in the so-called 'initial settlement', and its early origin is confirmed by stratigraphy: pottery of the first group was found in the ruins of mud-brick houses erected on virgin soil or in the next strata" (Sedov 1996: 69; see also notes 165–167 herein).

161 "[A] certain but not very close resemblance" (1996: 74; 1997: 45). "But again, like in the case of Palestinian pottery, these analogies [to Midianite pottery] do not show the coincidence in forms of vessels but only common similarities in the mode of painted decoration" (Sedov 1996: 76). This was followed on the same page of the German version (1997: 45) by "All in all it seems that the painted Hadramawt pottery is essentially similar to the Palestinian Late Bronze IIB pottery. The similarity is manifest in both the shapes of the vessels and in the subjects of the decoration."

162 "[C]lose only in a few forms...or some details like the cross-section of the rim (Sedov 1996: 72).

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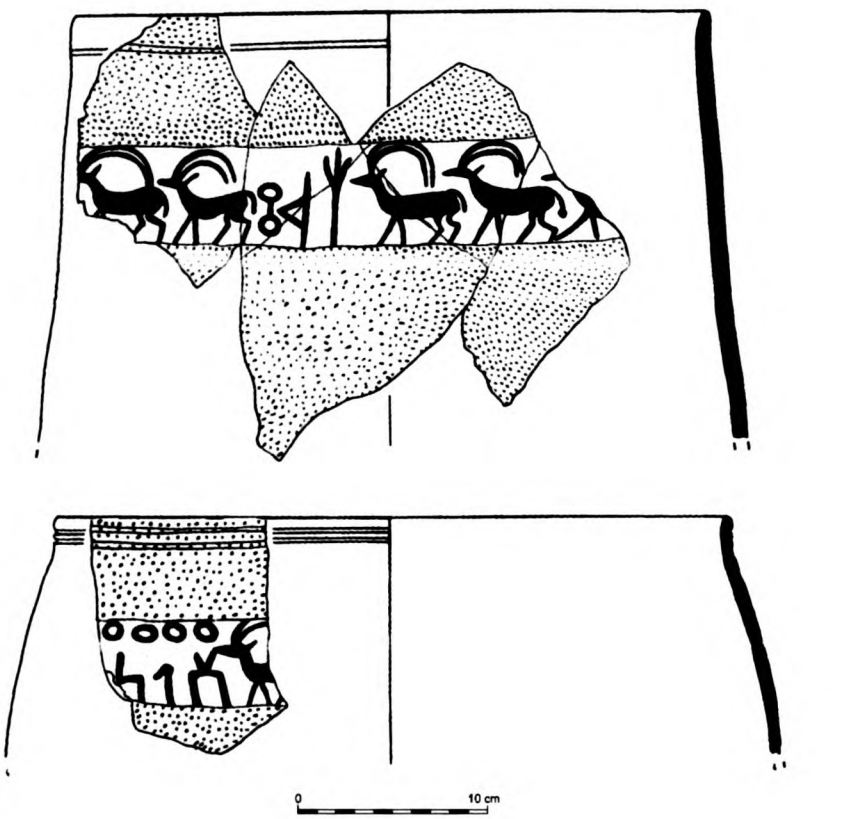

Fig. 23. Raybûn, sherds with painted inscriptions.

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mix was specifically referred to as suggesting a mid-second-millennium date for the painted pottery.[163]

Organic material from the oldest levels was analyzed at St. Petersburg and Moscow, yielding eight uncalibrated ^{14}C dates (Sedov 1996: 79; 1997: 46):[164] For the readers' convenience I have added in square brackets the dates calibrated with the OxCal program v3.8, at 1σ.

1420 ± 70 BCE (1996, 1997)[165]	[1880–1840: 10%, 1820–1790: 3.4%, 1780–1620: 54.8%]
1030 ± 140 BCE (1996)[166]	[1430–1050: 68.2%]
825 ± 100 BCE (1996)	[1130–890: 60.3%, 880–830: 7.9%]
760 ± 50 BCE (1996)	[970–950: 6.2%, 930–830: 62.0%]
730 ± 50 BCE (1996)	[920–825: 68.2%]
700 ± 160 BCE (1996; 1997)[167]	[1150–750: 58.1%, 700–500: 10.1%]
520 ± 140 BCE (1996; 1997)	[800–500: 60.6%, 490–480: 1.1%, 470–410: 6.5%]
460 ± 150 BCE (1997)	[770–400: 68.2%]

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163 "Furthermore, it is quite possible that as a result of future investigations this dating [to the last quarter of the second millennium] will have to be pushed back, even considerably, say to the middle of the second millennium or even earlier. We see the reason for this assumption in the results of our investigations at Raybûn XX–XII = Safa II. Painted Raybûn pottery was found there in 1991 with obsidian tools and stone tools in a layer which was *directly above an unquestionable Neolithic layer* [italics Sedov], very rich in finds" (Sedov 1997: 46–47).

164 1996: "There are several uncalibrated dates of some charcoal samples from different excavated areas at the Raybûn settlement". 1997: "For the moment we possess four uncalibrated dates." Three of the eight dates were listed in both the 1996 and 1997 publications, four in 1996 only, and one in 1997 only.

165 "[F]rom the strata at the bottom of the stone foundation of Building 3 in the south-western part of the settlement [i.e., Area II–III, *B.S.*]. This building, probably the oldest part of the big temple *Rḥbʾ* dedicated to the goddess *ḏt-Ḥmym*, was erected on the ruins of some early structures" (Sedov 1996: 79).

166 This sample and the three following ones "came from the 1991 sounding in Area IV, from the ruins of a mud-brick house built above the remnants of the 'early buildings' uncovered in the south-western corner of the settlement (Fig. 1) and from the strata above it. These 'early buildings' were erected on virgin soil and should be dated one or two centuries before the first radiometric date" (Sedov 1996: 79).

167 According to Sedov 1996: 79, this sample and the following one "were collected from the different strata at Area II–III where the remnants of mud-brick houses connected with the burnished red slip and red-painted pottery have also been discovered. Perhaps these dwellings could be referred to as the final stage of the early period of occupation." According to Sedov 1997: 46, this sample and the two following ones come from various strata in Area II, where the pottery from the mudbrick houses has a general resemblance to that of Stratum III in Area IV.

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The uncalibrated determinations were employed by Sedov to corroborate the dating *ca.* 1300–700 BCE suggested to him by the pottery comparisons mentioned above. The written material, however, was found only in the latest phase of this continuum or on the surface. Noticing the disagreement, Sedov sought to raise the date of the pottery to the second millennium via the sherd inscriptions by embracing Lundin's ultra-high chronology of the South Arabian alphabet (Sedov 1996: 79; see Fig. 23 herein):

> The presence of these inscriptions and graffiti testifies that such early dates like, for instance, the 13th century B.C. for the painted and red burnished slip pottery are doubtful. But one should pay attention to the archaic forms of the letters *alif* and *ḥ* in the inscriptions.

A year later Sedov (1997: 46–47) elaborated further on this notion:

> But such a ductus [of the *alif* and *ḥ*] is according to A.G. Lundin *characteristic for the 'South Semitic' script of the second half of the second millennium B.C.E.*, for which only the South Arabian script (including Hadramawtic) of the *later* monuments is as yet known [italics Sedov]. Such a ductus is thus theoretically admissible for the most ancient inscriptions from the Hadramawt. In that way the appearance of the pottery of the first group in the Wadi Hadramawt can be attributed to the last quarter of the second millennium.
>
> On Lundin's arguments, best forgotten, see p. 110.

Synthesis. Palestinian and 'Midianite' ceramic analogies led Sedov to date the beginning of the painted Raybûn pottery to the 13th century (1996: 77–79) and to seek the origin of the second-millennium Hadramawt population in northwestern Arabia (1996: 86). The lower end of the painted pottery was set towards 700 BCE because the second group of pottery, which replaced the first, was thought to have begun then (Sedov 1996: 82). This change was considered a signal of the submission of the Hadramawt to Sabaean rule (Sedov 1996: 84–86). And on the strength of the pottery comparisons just mentioned, of the uncalibrated radiometric dates, and of Lundin's theories, the beginning of the South Arabian alphabet was assigned to the fifteenth–thirteenth centuries.

2.1.3 Hajar Ṣurbân

The two adjacent sites in the Jabal an-Nisîyîn, dubbed Hajar Ṣurbân 1 and 2, were studied by a French mission (Breton and Darles 1994) following illicit digging. The largest pottery assemblage, twenty-two whole vessels, dug up by the local inhabitants (*ibid.*: 47, 49–50) includes a three-handled cooking-pot with an inscription incised before firing (SUR-2-14; *ibid.*: 50; Bron 1994; colour photograph in Robin and Vogt 1997: 106; Fig. 24 herein). The assemblage was compared to the pottery from the deep levels, Levels R–K, at Hajar Bin Humayd, with the conclusion that it "belongs

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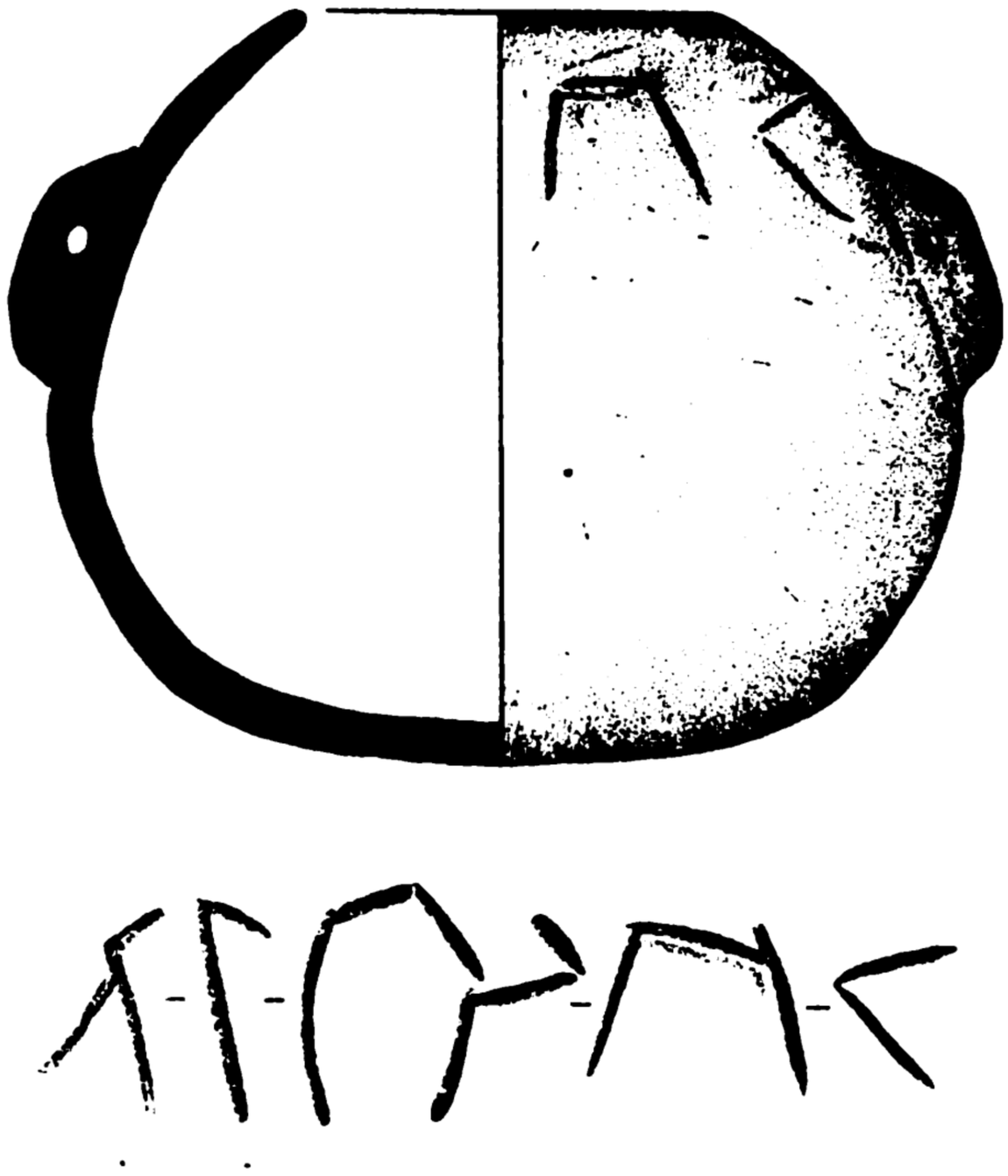

Fig. 24. Hajar Ṣurbân, cooking-pot with incised inscription.

to the eighth–sixth centuries BCE at least, but possibly even before" (Breton and Darles 1994: 51).

2.1.4 Al-Midaman

Among all Ṣubr culture sites,[168] the only one from which an inscription has been published so far is al-Midaman, a burial site situated near the west coast, roughly halfway between Hodayda and Mokha (Keall 1998; 2000). The Midaman inscription was incised before firing on what Keall termed a 'fenestrated cover' (Fig. 25; for an analogous complete example from Ṣubr see Vogt and Sedov 1997: 45 bottom right). "The pottery is...with some similarity of form to the al-Ṣubr corpus which, as...Vogt reported..., can be placed between 1300 and 900 BC on the basis of radiocarbon

[168] The Late Bronze Age Ṣubr culture of the southern and western Yemeni coasts differs from the cultures of the highlands and desert fringes. It is named after the key site, situated 20 km north of Aden (Vogt 1997; Vogt and Sedov 1997; 1998; 2001). It was chiefly the uppermost layers that were excavated; underneath are several additional metres of settlement debris (Vogt and Sedov 1998: 265). From the upper layers come 25 organic samples that yielded calibrated ^{14}C dates between the fourteenth and ninth centuries (Vogt and Sedov 2001: note 19). The end of the Ṣubr culture, and of the Late Bronze Age in the Tihâma, is understood as the elimination of this culture by the more advanced highland people *ca.* 900 BCE (Vogt and Sedov 2001: 289); alternatively in the eighth–sixth centuries (Görsdorf and Vogt 2001a: 1359, 1360).

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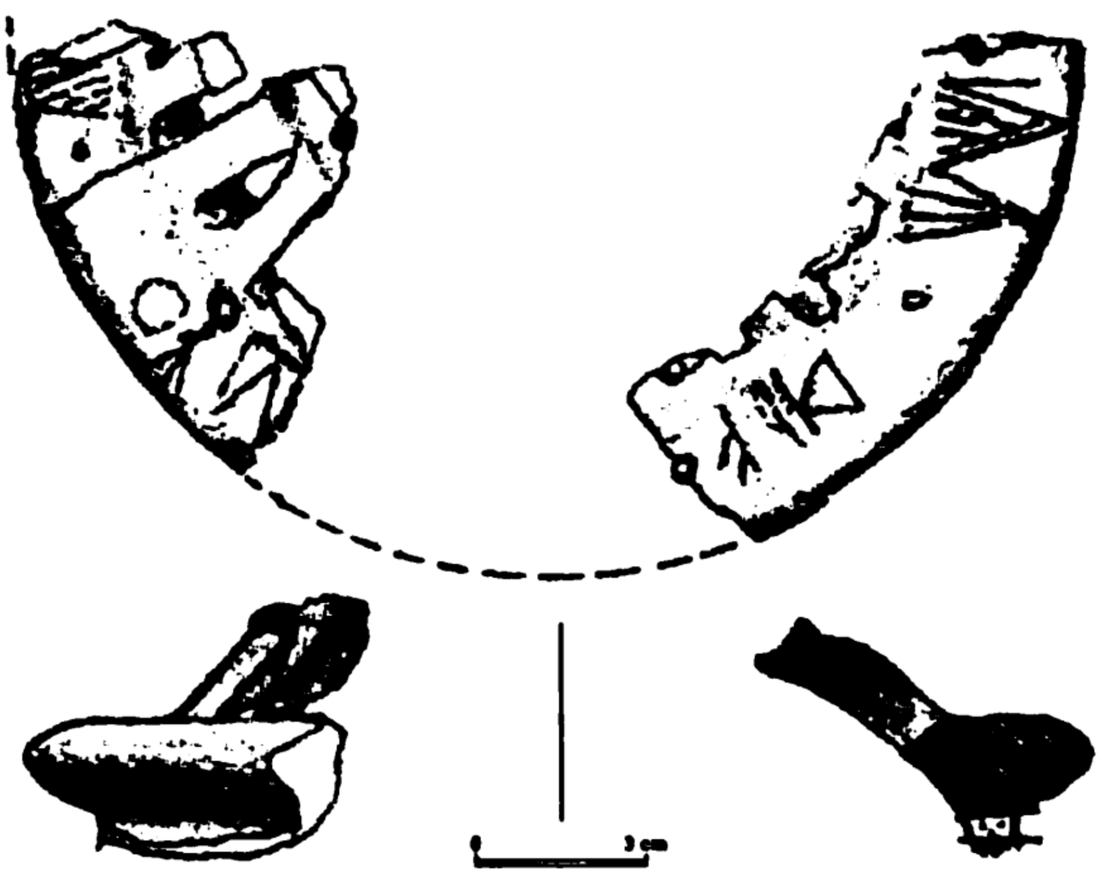

Fig. 25. Al-Midaman, inscription on openwork vessel or lid.

dates. It is the appearance at al-Midaman…of…proto-Sabaic letters in one example, that argue[s] most for the more recent date for this material…" (Keall 1998: 143). The specific context of this piece was not mentioned, but Keall (*loc. cit.*; see also 2000: 725) observed that "[e]rosion has resulted in much of the pottery being deposited in…tertiary contexts, but the large sherd size and well preserved state…of many pieces indicate that they are not far from their original context." As the Ṣubr culture is considered illiterate, the Midaman inscription, though incised on an apparently local vessel, is perceived to be the result of highland influence.

2.1.5 Early monumental inscriptions

The construction of the earliest phase of the ʿAthtar ḏû Riṣaf temple at es-Sawdâʾ, with its monumental architecture and art, was assigned to the eighth century by the pre-standardization script of its monumental inscriptions (Fig. 26; Breton 1990; 1992: 445; 1996: 98–101; 1997; Avanzini 1995: 15; Robin 1996c: 56–58). A single wood sample taken from within a wall associated with the inscriptions yielded a calibrated ^{14}C date of 870 (788) 433 BCE (Breton 1996: 101). See Addenda, p. 183.

The earliest monumental inscriptions at other sites are likewise commonly assigned to the eighth century (e.g., Robin and Vogt 1997: 56, 81, 90, 122, 133, 135).

2.1.6 Comments on the published results of the excavations (Sections 2.1.1–2.1.5)

On the strength of stratified inscribed potsherds at Yalâ and Raybûn, which in turn were dated by a combination of radiometry, local and foreign pottery comparisons, and palaeography, de Maigret, Sedov, and other authors concluded that the genesis of the Arabian alphabet was an event of the late second millennium at the latest,

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and their view was quite widely accepted.[169] This view accords well with the mid-twentieth-century opinion that favoured a Proto-Canaanite and even Proto-Sinaitic origin of the Arabian alphabets. (My earlier objections to the arguments behind these conclusions and case for a much lower genesis of the Arabian alphabets are found in Sass 1985: 215–217 = 1991a: 86–88.) Returning to the high dating of the South Arabian alphabet based on the results of recent excavations in Yemen, closer scrutiny, I believe, shows flaws in each of the criteria just mentioned and leads to a setting in the early first millennium instead:

Radiometric dates. What support can be found for a mid- to late-second-millennium emergence of the South Arabian script in the 1240–800-BCE calibrated radiometric range for Level B at Yalâ and uncalibrated 1300–700 at Raybûn? Calibrated, the range of the latter is mainly 1700–880. At Yalâ three inscribed sherds were attributed to the stratum thus dated, but the life-span of 440 years assigned to this level possibly indicates that something is amiss. At Raybûn the calibration requires first and foremost a re-evaluation of all the excavators' conclusions that were founded on the uncalibrated results. However, most inscribed Raybûn sherds in any case are surface finds; the remainder come from the very latest phases, so that they possibly date to the early first millennium. Besides, the fact that the present Yalâ Level B and Raybûn analyses were often carried out on samples of wood may account for the high top end of the said time ranges.[170] If the pottery analyses are added, the situation appears as follows:

A material culture 'stagnation'? At Yalâ, a pottery uniformity of 440 years was assumed, based on the 1240–800 radiometric dates of Level B. At Raybûn, the proposed range of 600 years (1300–700 BCE) for the painted 'first ware' must be raised to 820 years (*ca.* 1700–880) when the radiometric dates are calibrated. In Levantine terms the latter would be like lumping together the Middle Bronze, Late Bronze, Iron I and beginning of Iron II. At Yalâ the range, while smaller, is also clearly unfeasible. In most of the Near East such slow development is unheard of after the fifth millennium. Moreover, a statement seemingly contradicting the conclusions of the Raybûn excavators and in line with those offered in the present

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[169] For instance in published overviews: Robin dated the emergence of the Arabian alphabet first (1996a: 1208–1209) to the ninth–tenth centuries, then (1997a: 63) *ca.* 1000–1200, apparently following the Yalâ and Raybûn excavators. In the same spirit, Bron (1997: 55 and 57 respectively), in the captions to the photographs accompanying his article, dated the Raybûn sherds to the twelfth–tenth centuries, and the Yalâ sherds to the twelfth–ninth. Similarly Macdonald 2000: 32.

[170] In some Yemeni sites the preservation of wood is excellent, as demonstrated by Kamna 5, a monumental inscription on a wooden beam (Robin 1992: 172). The re-use of age-old beams if they were in good condition was obviously practised also at the time with which this chapter is concerned.

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Fig. 26. Es-Sawdâ', monumental inscriptions (continued on next page).

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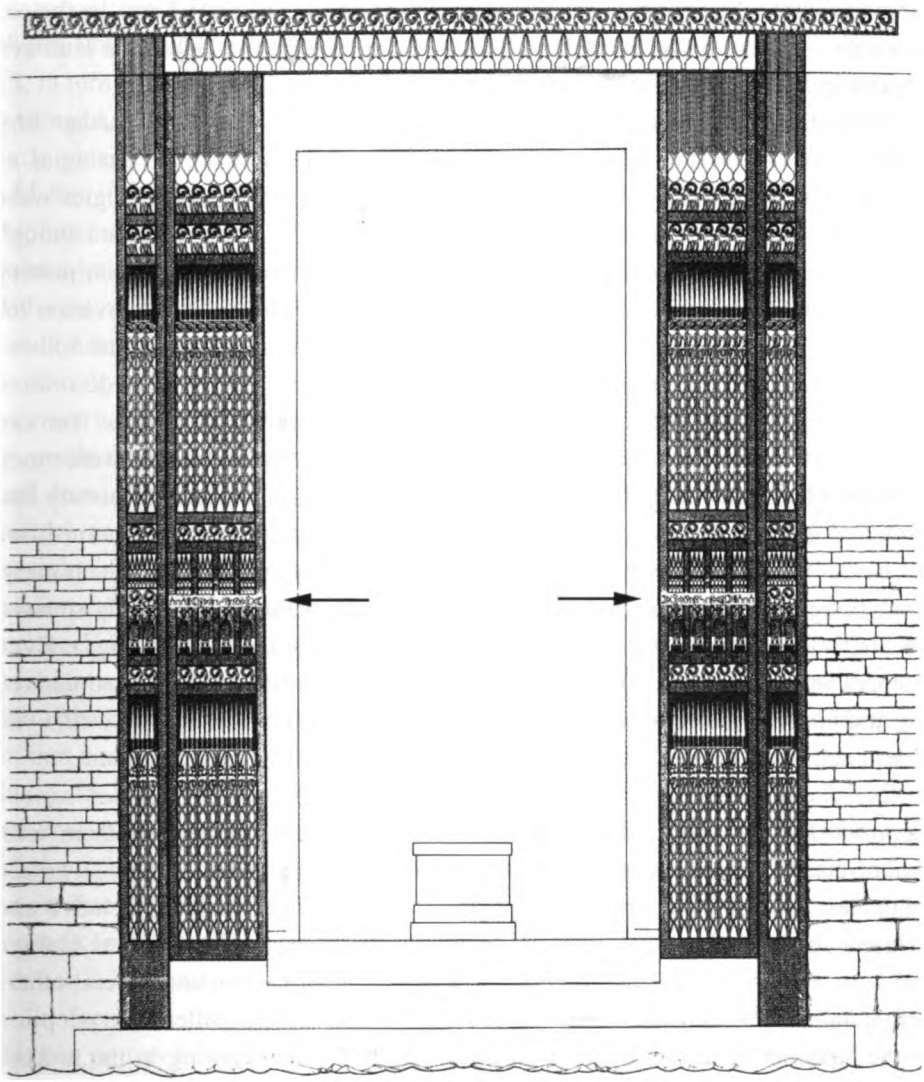

chapter is to be found in Görsdorf and Vogt 2001b: 1364: "It [the Marib Period I pottery—*B.S.*] matches well with ceramics from the early occupation at Raybun in Hadramawt thus giving us a date during the early 1st millennium BC." Most radiometric dates of Marib Period I cluster around 800 cal BCE (*ibid.*: 1365).

Hajar Bin Humayd pottery comparisons. The ceramic dates for Yalâ and other sites were founded in part on the sequence of Hajar Bin Humayd. To my mind a re-evaluation of the radiometric dates and material culture of the latter site, that takes into consideration recent developments in the chronology and pottery typology of the Levant, is indispensable before the results of the excavations at this key site

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can be profitably used again. Calibrated with the OxCal program v3.8 at 1σ the old radiocarbon dates of 2807 ± 160 and 2690 ± 100 B.P. from a Hajar Bin Humayd wooden beam yield results of 1220–800 and 1000–780 BCE respectively.

Raybûn comparisons with Palestinian pottery. It is doubtful whether any archaeologist of the Levant would concur with the Late Bronze Age analogies as presented in Sedov 1996 and 1997, all the more so as these very analogies were thought to cover the entire time-span until *ca.* 700 BCE. And important though Gezer is in Palestinian archaeology, no one would rely predominantly on pottery comparisons from Macalister's notoriously complicated century-old excavation for a chronological discussion. Some remarks on Sedov's Palestinian analogies follow: The repertoire of the shapes is unmistakably dissimilar. As regards decorative friezes, they are common in the pottery of various cultures and periods. Nor can images of ibex serve as a chronological marker; in southern Arabia they were much more popular in other media centuries after their numbers on Palestinian pottery had dwindled from the late-second-millennium high. Likewise, red slip and burnish are not particularly typical of the Palestinian Late Bronze Age, nor of Midianite pottery. If anything, they characterize Iron II, the ninth–sixth centuries, as already stressed for Hajar Bin Humayd by Van Beek (1969: 356, 'traditional' chronology). Even though I would hesitate to put too much faith in the comparisons with Palestinian red slip and burnish, it should be noted that in other parts of the Near East as well, even Urartu and Iran, this surface treatment is typical of the early first millennium.

The Raybûn inscriptions on painted pottery, and Lundin's theories on the alphabet. Picking out random Arabian letter shapes and juxtaposing them with Proto-Sinaitic, Proto-Canaanite, Ugaritic and other script signs will obviously yield a hopeless mix of meaningful and accidental resemblances. Heeding relative and absolute chronology and paying attention to essential aspects of the general Arabian and Near Eastern picture would perhaps have dissuaded Lundin and several others before and after him from attempting to justify a mid-second-millennium adoption of the alphabet in Arabia by such comparisons, and from ignoring in the process significant letter shapes which clearly indicate a first millennium adoption.[171] Lundin's theories are hardly a credible means for raising the date of the Raybûn painted pottery to the second millennium.[172]

The palaeography of the Yalâ sherds. According to the excavators, the Level

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[171] Cf. Sections 2.2.1 and 2.2.2, e.g., p. 120.

[172] The brilliance of Lundin's identification (1987) of the Beth Shemesh tablet as an abecedary of the *hlḥ* order (cf. Sass 1991b: 316) is recognized by all. Unfortunately this scholar's other perceptions of the development of the alphabet (e.g., 1982/1985), including an ultra-high Arabian chronology, are mostly unsupported by the evidence (cf. Sass 1991a: 29, 86–88). See meanwhile Lundin 1991 and 1997 with similar views.

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A inscriptions belong to the eighth–seventh centuries. To this level were assigned sherds with well-developed cursive letter shapes, like the open *mim* in Yalâ 4 and 18, in turn linked by Ryckmans (2001: 226–227) with his stage II of the 'minuscule' writing. At Yalâ this *mim* (if correctly identified) could thus indicate either a very early emergence of the said form or a too early attribution of the sherd. I should add that the sherds from Yalâ Level B contain together just 3½ letters (5½ if duplicates are counted), with no palaeographic development noticeable in the Level A inscriptions: The angular *ra* from Level B recurs in Inscription 10 from Level A; different *ya*s (if correctly identified), but not in the least more advanced, are found in Level A Inscriptions 11 and 12.

But by no means should the above comment diminish the value of the pioneer work done by the Russian–Yemeni expedition and other archaeologists in southern Arabia. It illustrates rather the preliminary state of Arabian Bronze and Iron Age archaeology today. The study of the Yalâ and Raybûn material culture and its interconnections has barely begun, but faster progress is indeed in sight.

Summing up the dating of the inscribed sherds. The very same data interpreted by the Yalâ and Raybûn excavators as supporting a date for the genesis of the Arabian alphabet in the fifteenth–twelfth centuries may on reflection exclude this dating categorically and indicate the early first millennium instead. It could be that a re-evaluation of the relevant Yalâ and Raybûn levels and their radiometric dates, in addition to a reconsideration of the pottery comparisons, will elicit either a further subdivision, or a reduction of the said dates to the final years of the life-span originally put forward by the excavators. This would indeed be in line with the West Semitic epigraphic comparisons, the archaic Arabian legends on datable Mesopotamian seals, and the general Near Eastern picture (see Section 2.2 below).

In fact even the dating evidence already available for the Yalâ Level B and Raybûn pottery inscriptions does not to exclude an eighth-century date; the range is wide enough.[173] The same could possibly be said for the Hajar Ṣurbân inscription. Regarding Midaman, it could be claimed on account of its Ṣubr connections that the inscription from this site is earlier, yet as Görsdorf and Vogt concluded (see note 168), the end of the Ṣubr culture may be centuries later than the originally-postulated 900 BCE. Despite this uncertainty I still favour the ninth century for the earliest of the above inscriptions for reasons presented on pp. 113–114.

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[173] The differences between the inscriptions from the two sites notwithstanding—Yalâ cursive and rather careless, Raybûn neat and almost monumental. On the dating of the Yalâ inscriptions see further above, on the Raybûn inscriptions—p. 117.

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The origin of the population. Garbini (1992) was of the opinion that some of the Yalâ inscriptions contained exclusively North Arabian personal names. I am less certain: The inscriptions in question are mostly fragmentary; some, moreover, may be read otherwise or the words divided differently. Nevertheless, there is a fairly broad consensus that the carriers of the literate civilization in South Arabia were immigrants from the north.[174] Obviously, population movements did occur in human history, but does the social and cultural change in southern Arabia in the early first millennium require völkerwanderungen as its justification? As long as there is no specific evidence from Yemen for such a phenomenon, similar weight, if not more, should be accorded to the possibility of a local development,[175] in conjunction with Levantine trade-related and possibly other interactions. For a balanced overview see Robin 1996a: 1109–1110.

Conclusions. Excavations in Arabia are just beginning to bear fruit. Radiometric dates are just beginning to accumulate, and the chronological picture they provide for stratified inscriptions will no doubt become more focused with time. The construction of an Arabian pottery chronology is in an initial stage as well. The comparisons may in due course lead to more concrete results than the attempted analogies to the surface treatment and decoration of Palestinian pottery. It will not surprise me if these comparisons to the earliest Arabian strata with inscriptions point to early Iron II in the Levant, less probably late Iron I, but not earlier. Further, as demonstrated by the Beersheba and Aroer material (see note 178), Arabian finds will be increasingly identified in excavations in southern Israel and Jordan now that archaeologists are beginning to be aware of such finds. Finally, specialists invoking Levantine Late Bronze and Iron Age comparisons for Arabian archaeology and epigraphy will have to be familiar with the corrected Iron Age chronology of Palestine.

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[174] For instance Vogt and Sedov 1998: 276. They spoke of the Semites arriving during the second half of the second millennium, probably from northwestern Arabia, soon to become "a fully developed proto-South Arabian substratum" in the highlands. This may recall the long-held view about the appearance of the Israelites out of Sinai, by now long-discarded, or of the Aramaeans out of the Syrian Desert or Jebel Bishri—contested on pp. 63–64; see also de Maigret 1996: 175 = 2002: 185: "[W]e must opt for an external origin of the South Arabians. Further north, in Mesopotamia, Syria and Palestine, we find that new populations arriving with quite different material cultures were commonplace and were, we might say, the rule."

[175] In the same vein it has been suggested by Audouin (1996: 142) that the decoration of architectural elements of early monumental South Arabian temples harks back to the adornment of temples of the second-millennium built of mudbrick or half-timbered. I may add that some of the intricate decoration incised in stone of the monumental temples is reminiscent of textiles, and probably imitates earlier wall hangings, or the tent-cloth of movable shrines. With the dry climate of the Yemeni desert fringe it is not impossible that such early tapestries will be discovered one day.

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2.2 THE ADOPTION OF THE ALPHABET IN ARABIA

The Levantine parallels for the earliest Arabian letter shapes straddle the transition from Proto-Canaanite to the next phase of the West Semitic alphabet, *ca.* 900–850 BCE (on the West Semitic chronology see Chapter 1). But as the comparisons are few, a wider uncertainty margin may be necessary—say a further 50 years in each direction or *ca.* 950–800.[176] Within the said range the time around 850 BCE seems to me the most likely. The foregoing refers to southern Arabia; virtually nothing is known about the alphabet in northern Arabia at that time, but I believe that the adoption of the alphabet in the north and in the south was simultaneous.[177] The rationale, non-epigraphic as well as epigraphic, behind these suggested processes is presented in Sections 2.2.1 and 2.2.2.

2.2.1 Non-epigraphic evidence

2.2.1.1 Archaeological dating of excavated inscriptions, stylistic dating of inscribed seals

The new excavations at Yalâ, Raybûn and other sites yielded archaic inscriptions on pottery from stratified contexts or from findspots that can indirectly be associated with such contexts. This is the main development since my earlier work, a most welcome addition to what has hitherto been known chiefly from Hajar Bin Humayd (cf. Sass 1985: 194–195 = 1991a: 33–35). At Sawdâ' monumental inscriptions came to light, written in an archaic or 'pre-classical' ductus, which can probably be dated to the early first millennium by associated ^{14}C.

With more excavated samples and a refinement of the radiometric chronology the difficulties encountered in dating the Yalâ Level B and Raybûn inscriptions will eventually be resolved and, I hope, lean towards the ninth century. On my differences with the excavators see pp. 106–112, also on the pottery comparisons.

Regarding Arabian inscriptions from other parts of the Near East datable by non-palaeographical evidence: In my earlier work I listed 18 unprovenanced seals attributable by their Mesopotamian or Levantine style mostly to the eighth–seventh centuries (1985: 202–207 = 1991a: 69–73). Between then and 2004 a few additional

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[176] The West Semitic comparisons pertaining to the genesis of the Greek alphabet are more numerous and conclusive (see pp. 140–142).

[177] Whether Arabians travelling in the Levant took the alphabet home with them or people from the Levant travelling to Arabia brought it there is unknown; the former possibility is perhaps the more likely.

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inscriptions from the same time became known.[178] The 1985/1991 picture remained unchanged, however, because only inscriptions of the ninth (or tenth) century would have the potential to transform our understanding of the genesis of the Arabian alphabet. With the possible exception of a single cylinder seal dated to the ninth–eighth centuries (Sass 1991a: 70), such have not yet been positively identified. Until the typological and radiometric datings of the excavated South Arabian inscriptions stabilize, the legends on eighth-century seals remain the earliest securely-dated Arabian texts.

In other words, no Arabian inscription has yet been dated with certainty to the ninth century, the time which I regard as the most plausible for the adoption of writing in Arabia. Should the adoption therefore be down-dated to the bottom end of the maximum range indicated by West Semitic palaeography, *ca.* 800 BCE? Not necessarily. It is true that in such a manner the earliest alphabetic writing in Arabia and in the Greek world would come very close in time (cf. Chapter 3, especially pp. 149–152). I am turning to the potential lesson to be learned from the Greek analogy. The range of dates provided by West Semitic comparisons for the adoption of the Hellenic alphabet is *ca.* 825/800–750. Had the present Greek archaeological evidence not rendered unlikely a date much earlier than 750, I personally would have opted for an adoption *ca.* 800 BCE. An analogous chronological verdict for Arabia (i.e., down-dating the adoption to the lower limit of the West Semitic comparisons, *ca.* 800 BCE) will impose itself only if (or when) the Arabian archaeological evidence for the period in question becomes as abundant as that now available for the Greek world and there would still be no inscription that could be dated earlier than the eighth century. As long as the question is open I continue to believe that secure evidence for a ninth-century dating of the earliest Arabian inscriptions, monumental and non-monumental, will eventually turn up. The mid ninth century remains my best choice for the adoption of the alphabet in Arabia because it is favoured by the West Semitic alphabetic comparisons (see p. 122; for the supportive Near Eastern picture see pp. 118–119).

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[178] The site of Beersheba yielded several Arabian finds, including alabastra and a short inscription incised on a small limestone object of uncertain use (Singer-Avitz 1999: 46–52). Most finds come from the final and richest Iron Age layer, Stratum II, probably destroyed in the late eighth century (*NEAEHL*: chart on p. 173) or at any rate not much later than the early seventh. The stratigraphic position of the Arabian finds from Aroer, including two fragmentary inscriptions incised on local pottery vessels, is currently being evaluated, as these finds are processed for publication by Yifat Thareani—who kindly gave me the information. The excavators assigned the earliest phase of the site to the late eighth century (*NEAEHL*: 90), whereas Thareani, following Singer-Avitz (1999: 56), would date the two deepest strata III and IV to the eighth century, equating Aroer III with Beersheba II. For another inscription from an excavation in Iraq, incised on a pottery jar after firing, see Robin 1994: 86–87. The jar, which contained a burial, was dated by its excavator to the eighth century. A few more unprovenanced inscribed seals were also published, e.g., Christie's sale catalogue 2001: no. 530.

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2.2.1.2 The general South Arabian picture and the Assyrian synchronism

The carriers of the South Arabian culture. As noted on p. 112, the Arabians who adopted the alphabet did not have to be newcomers, from the north or from elsewhere. Nothing in the evidence rules out a gradual ripening of indigenous developments, coupled, to be sure, with trade-induced inspiration from the Levant,[179] which culminated in a literate society with evolved governmental institutions and monumental architecture, art and inscriptions in southern Arabia.

The Assyrian synchronism. I come now to von Wissmann's synchronism of Itaʾamra mentioned by Sargon in 716 BCE with Yiṯaʿʾamar Bayyin son of Sumuhuʿalî, and of Karibilu mentioned by Sennacherib in the 680s with Karibʾil Watar son of Ḏamarʿalî. This Assyrian synchronism, and the absolute dating it lends to the unification of southern Arabia under Sabaean hegemony and to the 'classical' phase of monumental writing (Robin 1996a: 1121), is meanwhile regarded by many authors as practically certain. In the eighth century, the reasoning continues, developed pre-unification states existed in southern Arabia (e.g., Robin 1997a: 62; 1997c; Breton 1997: 137; Nebes 2001: *passim*).

The synchronism is evidently most welcome in the general dearth of reliable historical anchors. And the upgrading in the Assyrian sources of the rulers from a mere "Sabaean" (Itaʾamra) to a "king of Saba" (Karibilu)—who, moreover, bore not tribute but offerings added to the foundation deposit of a new *bît akîtu*—is a point in favour of the identification of the latter as the great unifier (cf. Robin 1996a: 1118). But as with the equation of Sasnuri of Gozan with the Assyrian eponym for the year 866 BCE (see pp. 93–94) one should not lose sight, because the proposed identification is so convenient, of what this identification really is—plausible but for now unverifiable (Sass 1985: 290–291 = 1991a: 83).[180] Caution in this respect was indeed recommended by Robin, though the only alternative he foresaw was a slight lowering of the dates.[181]

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[179] See pp. 118–119.

[180] For von Wissmann (e.g., 1976: 329–330) the synchronism together with the Hajar Bin Humayd monogram constituted the foundation of his chronology. My dating of the South Arabian alphabet, while roughly coinciding with von Wissmann's, depended at the time not on the synchronism but chiefly on West Semitic palaeographic comparisons, then the Mesopotamian glyptic and Hajar Bin Humayd (and, alas, the Queen of Sheba narrative—see note 186).

[181] Robin 1996a: 1120: "[T]hese arguments do not permit to regard the identification as assured. The archaeological dates appear to contradict Jacqueline Pirenne's thesis: It is no longer possible to date Karibʾîl the Great to the second half of the fifth century and maintain that the monumental South Arabian writing had taken Greek writing as its model. But this evidence implies only a date earlier than the beginning of the sixth century: Karibʾîl the Great may have reigned 80 years after Karibilu who, in this case, would have been one of the Karibʾîls mentioned in the genealogies of the 'Eponym Lists'. Despite this persisting uncertainty we retain the identification as a working hypothesis." On some hesitation as to the validity of the other synchronism, Itaʾamra = Yiṯaʿʾamar Bayyin son of Sumuhuʿalî, see Robin 1996a: 1121.

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To this I wish to add considerations for the raising of the dates. (The coming paragraphs were toned down following a long conversation with Christian Robin in September 2003.)

Dating the earliest South Arabian states and monuments. If Karib'il Watar, in line with the synchronism, lived in the early seventh century, the emergence of states and monuments in southern Arabia could hardly have preceded the eighth century (this is the present consensus). An earlier dating would in such a case not only denote that Arabian states had existed for up to two centuries prior to the unification, but also that epigraphical Stage A had the same duration. But even though the great inscription of Karib'il (cf. Nebes 1997) may well indicate a long preceding phase of rivalries and conflict,[182] a 200-year period is almost certainly too long for Stage A of the script (second possibility in Table 7).

On the other hand it stands to reason that once international commerce took off in earnest in the ninth century (p. 118), the far-travelling Arabians were quick on the uptake in their dealings with the Levant and Mesopotamia. Just as they observed and eventually adopted (or adapted) the alphabet from their customers in the north, the new (ninth-century) Levantine political formations and the monuments that go with them will not have escaped the Arabians' notice. In other words, raising to the ninth century the earliest Arabian states with their monumental buildings and art is consistent with (though obviously not proven by) the Near Eastern evidence.[183] This in turn would make the earliest monumental inscriptions in southern Arabia practically contemporary with the adoption of the alphabet itself.[184]

If a 200-year lifetime for Stage A of the monumental script appears too long, the period of pre-unification states and pre-standardization scripts can be moved up to the ninth century only by sacrificing the Assyrian synchronism. If so, it may be proposed

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[182] The time necessary for the various South Arabian states to establish themselves, then for the kingdom of Awsan to affirm its supremacy. I owe this comment to François Bron.

[183] See p. 114 on the similarities and differences in the parallel process among the Greeks.

[184] See already Knauf 1989: 79, 86 (this work came to my attention after the publication of Sass 1991a): "[T]hese two phenomena [the dissemination into Arabia of statehood and of writing] are closely linked.... Von Wissmann, whose chronology places the earliest Sabaic monumental inscriptions in the 8th century, is correct. His assumption that the Sabaic state, or even empire, existed as early as the tenth century BC is, however, as untenable as the South Arabian chronology of J. Pirenne. The increasingly monumental character of the early Sabaic inscriptions reflects the beginning of Sabaean state formation as much as their content." Whereas I concur with much of the above, my reconstruction of the nature of the "migration of the script" to Arabia and its date differs considerably from Knauf's. On the lack of need for a long formative period see p. 126.

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TABLE 7. THE TIME-LAPSE BETWEEN THE EARLIEST SOUTH ARABIAN STATES AND KARIB'IL WATAR

	First possibility (present consensus)	*Second possibility (unlikely)*	*Third possibility*
Karib'il Watar	7th century	7th century	8th century
Earliest states and monumental writing	8th century	9th century	9th century

that Karib'il Watar be raised to the eighth century, and with him the unification of southern Arabia and the birth of the 'classical' script (third possibility in Table 7). In this case Sennacherib's Karibilu will have to be equated with a later Karib'il.

Lacking concrete dating evidence for such an upward revision of the earliest states and monumental writing, it may seem to South Arabian specialists more prudent to stay for the time being with the synchronism, despite its own manifest uncertainty. But the Assyrian synchronism may have yet another problematic aspect: It would require that the Raybûn pottery inscriptions, themselves of a ductus very close to monumental Stage A (see note 159 and Fig. 23), also be down-dated to the eighth century. In theory this could be countered by devising a script phase 'A0' which will accommodate the Raybûn type of *alif*, missing in the most archaic South Arabian monumental inscriptions (though see note 202). Yet presumably even such a feat would add no more than a few decades. Certainly from a Levantine standpoint my preference is the slightly higher alternative: the most-archaic-looking Arabian inscriptions, both monumental and cursive, reflect a West Semitic prototype of the ninth century (see p. 122).

In other words, assuming a delay of 100–150 years (or more) between the adoption of the alphabet and the emergence of the first monumental inscriptions in southern Arabia would require believing in an equally-long palaeographical standstill. This, barring evidence to the contrary, seems to me rather forced. (Just as Stage A could hardly have lasted as long as two centuries, so is a purported pre-unification Arabian alphabet, non-monumental yet practically identical with monumental Stage A, unlikely to have remained frozen for a century and a half.) But with the imprecision of palaeographical dating a shorter delay may be potentially acceptable (for it is impossible to detect), say an advent of the South Arabian states with their monumental architecture and inscriptions a few decades after the adoption of the alphabet—still before the end of the ninth century.

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The palaeographical evidence seems to indicate that the alphabet was adopted in northern and southern Arabia at the same time, about 850 BCE. For the south the foregoing reconstruction may favour an adoption of the alphabet not by an existing state but rather by traders, not too long before—and probably leading to—the emergence of the first state administrations.[185] In the north the alphabet took root under still unknown but certainly quite different conditions.

2.2.1.3 Fertile Crescent picture

As noted, it is the Near Eastern situation in the ninth century that brought to my mind the above question of raising the dates of the monumental Arabian script. With the ascent of the Assyrian empire and the foundation of numerous Levantine states the incense trade must have attained an unprecedented high. That the ensuing accumulation of wealth and world wisdom led to fundamental changes in the South Arabian society is undisputed. As the developments in the Levant show, statehood and monumentality did not require centuries to spread; only a few decades sufficed. In such a fashion the earliest South Arabian states could constitute an indirect extension of the ninth-century north Syrian 'domino effect' (see p. 65). Indeed, Arabian incense and luxury products are documented in Assyrian sources since that century (see below).

On the other hand, there is little or no evidence for Arabian Late Bronze Age trade with the Levant, certainly not on a first-millennium scale. This could bespeak a slightly later deployment of the camel as a long-distance pack animal. In potential it is thus towards the end of the second millennium that the long-range incense trade received this very important boost, but in the first two post Bronze Age centuries, the trade could hardly have reached a sizable volume. The Suhu inscription of the eighth century (Cavigneaux and Ismail 1990) shows Hindanu to have been an outlet for the South Arabian trade; so do earlier Assyrian sources since Tukulti-Ninurta II (890–884), but not yet under Tiglath-pileser I (1114–1076). This led Liverani (1992: 112) to seek the beginning of the Arabian trade with Mesopotamia "at some point between 1075 and 890,"[186] which in turn coincides with the time suggested by Finkelstein (1988 and 1995: 121–123) for the beginning of the Arabian trade with the Levant. According to the chronology correction this development and the revival in

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185 In my earlier work (1991a: 93) I advocated as the sole possibility the adoption of the alphabet by an Arabian state administration. This stemmed from following the 'traditional' Levantine chronology as well as the biblical narrative about a Sabaean kingdom of the tenth century.

186 The one point of Liverani (*loc. cit.*) with which I cannot concur is that on King Solomon and the Queen of Sheba. In my earlier work (1991a: 2–3, 31) this story is referred to as a historical source, but as I realized soon afterwards it is not to be employed as evidence for the tenth century in which it is set—cf. among many others Knauf 1994: 121; 2002: note 4; Briend 1996: 1045–1046; Gilliot 1997; Na'aman 1998: 72–74. Among the advocates of a more conservative view Kitchen 1994: 190 and Lemaire 2002 may be mentioned.

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southern Palestine would date to the tenth century (compare p. 67 on 'New Canaan'). Late Iron I or the tenth century may indeed be the earliest time for which Levantine archaeological evidence indicates a realization of the said potential, if on a smaller scale than in the ninth century onwards.

In this fashion, the broader ancient Near Eastern picture appears to join the general Arabian picture and the West Semitic epigraphic evidence in its rejection of a second-millennium dating of the adoption of the alphabet in Arabia and in its support of an early-first-millennium dating.

2.2.2 Epigraphic evidence

2.2.2.1 The West Semitic prototype and the time of the adoption

Although the West Semitic parentage of the Arabian alphabet is generally acknowledged, the West Semitic shapes that specialists recognize in the early Arabian inscriptions differ.[187] I found the shapes of eleven Arabian letters to be reminiscent to varying degrees of their West Semitic counterparts—*ba*, *ǧim*, *dal*, *lam*, *mim*, *nun*, *ʿayn*, *ṣad*, *qaf*, *shin*, *taw* (chart in Sass 1985: 215 = 1991a: 86). Obviously the dating of the adoption is determined by the youngest among them. Arabian letters that look as if they originated in earlier West Semitic forms or stances, such as the shapes of *ba* and *ṣad* or the upright stance of the *shin*, should, against the background of the younger forms (see below), be regarded as post-adoption modifications that have resulted in chance resemblances.[188] Therefore, I now remove *ba* and *ṣad* (and add the D-shaped *fa* on the assumption that it is a pre-standardization form).[189]

Of the ten letters in question, the most decisive, by which the adoption may be dated, are early Arabian *lam* and *nun*, which had the West Semitic long-armed or medium-armed *lamed* and legless or short-legged *nun* as their prototypes. These two letters were already the focus of my earlier dating of the South Arabian alphabet (1985: 216 = 1991a: 87–88). But at the time the early South Arabian cursive was not yet available to me and, still obeying the 'traditional' chronology, I dated the same West Semitic comparisons *ca.* 1000 BCE—a century and more too high. The following changes should now be noted:

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[187] Robin 1996a: 1209: "[T]he Arabian alphabet displays a manifest Phoenician parentage: seven among the twenty-two Phoenician letters have identical form and value." Bron 1997: 55: "A certain affinity with the Phoenician alphabet has long been noticed: several letters in the two alphabets are either identical (*ʿ*, *t*) or show a great resemblance (*g*, *l*, *n*, *q*, *š*)."

[188] Sass 1985: 216 = 1991a: 88. On a similar process in the Greek alphabet compare pp. 143–145.

[189] The chronological implications of the link between this form of *fa* and the archaic Byblos *pe*, Ahiram's in particular, were referred to in Sass 1985: 214 and 1991a: 85 (still according to the 'traditional' chronology). This letter was inadvertently omitted from the chart mentioned in the main text, however.

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1. More than the angularity of the letter—i.e., single angle—as I held before (Sass 1991a: 87), it is the lengthening of the arm that links the Arabian *lam* to the early-first-millennium West Semitic *lamed* and sets it apart from the Late Bronze–early Iron I shape, coiled, hence armless and hardly angular. The lengthening of the *lamed*'s arm is known only since the Iron I–II transition, or a little before, as pointed out half a century ago by Milik and Cross (1954: 12; cf. Sass 1988: 124).
2. As to the Arabian *nun*, the cursive form with short leg may, by its West Semitic comparisons, indicate a time slightly later than the Iron I–II transition.

To these the absence of a pupil in the Arabian *ʿayn* could probably be added. In a putative prototypal Proto-Canaanite *ʿayin* of the thirteenth century or earlier the pupil would have been prevalent, but it became ever more scarce afterwards, finally disappearing in the ninth century. Note also the angularity of the Arabian *shin* as opposed to the West Semitic letter; the latter is typically rounded until the thirteenth century, and almost exclusively angular by the tenth, if not before (on the stance of the Arabian *shin* see above and Sass 1985: 216 = 1991a: 88).

Table 8 shows where along the evolutionary path of the West Semitic alphabet the shapes of *lamed* and *nun* are closest to the earliest Arabian *lam* and *nun*.[190] Incidentally, the Proto-Sinaitic *lamed* was also long-armed. But this form is unrelated to the issue at hand; it was replaced by the coiled *lamed* no later than the thirteenth century, and all that time the *nun* still retained its Proto-Sinaitic serpent shape. Both forms coexist in the Lachish ewer, dating to that century, and are evidently not represented in the early Arabian alphabets.[191] *This comparison alone suffices to show that the Arabian alphabet must be later than the thirteenth century.*[192] It advises against the search by Lundin and others for ever more archaic features in the earliest inscriptions (see p. 110): Obviously the oldest Arabian inscriptions cannot be more archaic than their early-first-millennium West Semitic prototype.

The transmission of the alphabet to Arabia must have been much later than the thirteenth century. Only the very latest among the Proto-Canaanite inscriptions,

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[190] The table contains a representative sample; the full picture may be consulted in Table 2 herein, in Sass 1985: Table 5 (p. 272) = 1988: Table 5 (unnumbered p. 184), and in Sass 1991a: Chart 2 (unnumbered p. 100).

[191] The Proto-Canaanite documentation is too poor to indicate how long before the thirteenth century the *lamed* lost its arm. But see now Sass forthcoming, in Addenda, p. 184.

[192] "It is impossible to accept the suggestions that the alphabet was adopted in southern Arabia in the 14th–13th centuries, if only for the reason that this would imply that South Arabian *lam* and *nun* had for about 500 years followed precisely the course of development of the Proto-Canaanite/Phoenician *lamed* and *nun*. In other words, no connection between the South Arabian *lam* and *nun* and 13th-century Proto-Canaanite *lamed* and *nun*, e.g., those on the Lachish ewer, can be traced" (Sass 1985: 216 = 1991a: 87).

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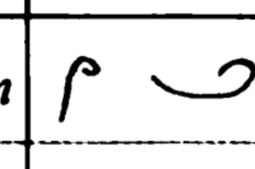
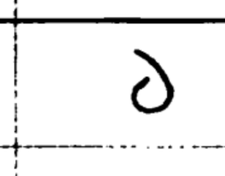
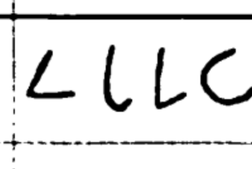
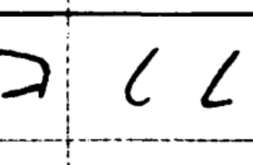
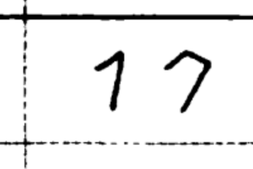
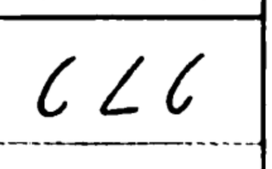
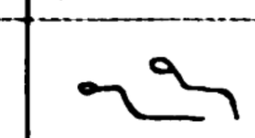
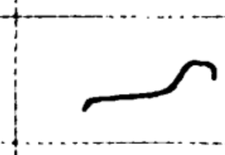
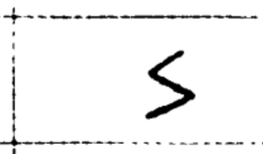
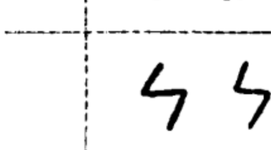
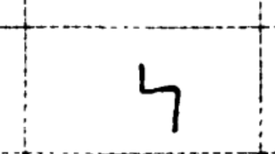
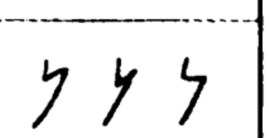
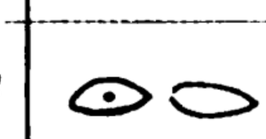
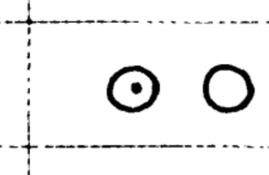
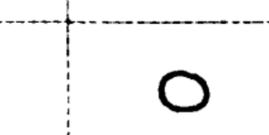
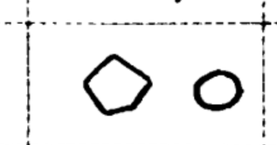
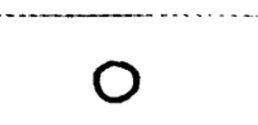
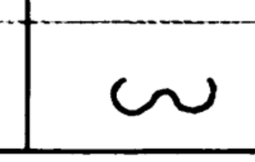
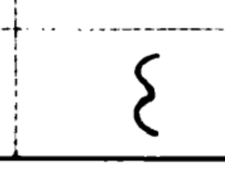
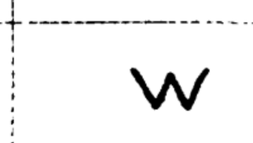
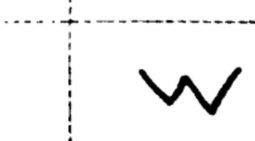
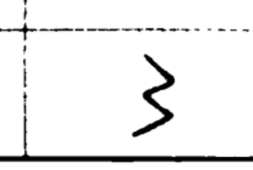
TABLE 8. DIAGNOSTIC EARLY ARABIAN LETTERS AND THEIR WEST SEMITIC COMPARISONS

	Proto-Sinaitic	*Lachish ewer (13th cent.)*	*Bowls, arrowheads (ca. 900)*	*Byblos*	*Yalâ, Raybûn, etc.*	*Kulamuwa, Hazael, Mesha (ca. 830)*
lamed / lam						
nun						
ʿayin / ʿayn						
shin						

the Tekke and Kefar Veradim bowls, and several of the arrowheads, contain a combination of a long-armed or medium-armed *lamed* and legless *nun*. (The el-Khadr arrowheads, which display the most archaic ductus in this group, have a *lamed* which is close to the coiled shape [Milik and Cross 1954: 12]). Judging by the bowls, this combination would date *ca.* 900–850 BCE, the top end perhaps extendable slightly by the arrowheads.[193]

However, since most Arabian letters underwent considerable modifications, it could be argued that the long-armed *lam* and the legless *nun*, too, are the result of local post-adoption developments that render the underlying West Semitic shapes unrecognizable. I do not think this very likely. That such was not the case may be indicated by the letters of the earliest-known South Arabian cursive texts, which appear to have retained a few more West Semitic features from the time of the adoption than their monumental counterparts. The more varied cursive may therefore add a measure of accuracy:

- The graphic variants of both the West Semitic *lamed* and South Arabian cursive *lam* are revealing. In shape—not in stance—the Yalâ *lam*s still follow quite closely the variants of the West Semitic model: long-armed, medium-armed, rounded, double-angled and single-angled—compare Fig. 21: 6, 9(?) and 11 with 7(?) and 19 (latter in Garbini 1992: 84). Similarly the most archaic among the palm-stalk inscriptions (Ryckmans 2001: 225–226 and Fig. 1).
- The West Semitic *nun* may have begun to develop a leg in the first half of the ninth century or slightly before (see on *mem* and *nun* on pp. 30–31). The Yalâ

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[193] My best guess for the beginning of the arrowheads—the lack of anchors to absolute chronology making certitude impossible—is *ca.* 900 BCE, or at most the second half of the tenth century (see pp. 43–44).

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*nun*s, too, display a short leg.[194] If this feature harks back to the West Semitic prototype, the adoption would have to be placed in the late tenth century or the first half of the ninth but not afterwards, for then the leg of the West Semitic *nun* becomes longer.[195]

Within the range of *ca.* 900–850 BCE (or at most *ca.* 950–800) indicated for the adoption by the West Semitic comparisons, the concurrence of the above letter forms appears to me to lend a certain edge to the mid-ninth century. At the same time several non-epigraphic Arabian indices combine to oppose a much earlier adoption (above). And in the second half of the ninth century the legless or short-legged West Semitic *nun* becomes rather rare, most examples already displaying a veritable long leg, which in turn cannot be reconciled with the shape of the Arabian *nun*.

2.2.2.2 The hlḥ *alphabetic order and the time of the adoption*

On account of the Beth Shemesh abecedary I understood the *hlḥ* order in 1991, and still do, as a second order of West Semitic origin.[196] The publication by Bordreuil and Pardee in 1995 of another *hlḥ* abecedary from the Levant, this time a decidedly long alphabet on a tablet from Ugarit, dates the said letter order no later than the thirteenth century, much earlier than any Arabian abecedary or indeed the Arabian alphabet. And it clinches the view of a West Semitic origin. Obviously the fact that the long *hlḥ* order is better documented in Arabia (and Ethiopia) at a much later time cannot in itself be used to argue the origin of this order in that part of the world.[197]

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[194] Such as in nos. 12 and 15. It should be remembered that these inscriptions come from the later Yalâ Level A (see pp. 110–111). The Level B inscriptions contain no *nun*.

[195] That in the monumental South Arabian *nun* both lateral strokes are strictly of the same length may in that case be the result of a modification for considerations of symmetry. The Greek *nu* also ultimately became N-shaped, but this resemblance to the Arabian form is clearly incidental: Unlike the Arabian letter, the earliest occurrences of Greek *nu* show that the West Semitic prototype was the later long-legged *nun*. Indeed a trend to clip some of the legs of the adopted West Semitic forms, or eliminate the legs altogether, can be observed in the Greek letters not long after the adoption (pp. 142–143 with note 230). A similar process did not take place in the Arabian alphabets.

[196] "[T]he Beth Shemesh tablet complicates the situation in the sense that the *hlḥ* order alone cannot be regarded anymore as automatic proof for a long alphabet and South Semitic attribution. We may as well be dealing with two contemporary Northwest Semitic alphabetic orders, employed in both the cuneiform and Proto-Canaanite alphabets, that started their careers as long ones, to be subsequently shortened" (1991b: 319). The question of why the West Semitic alphabet had two orders remains unanswered, however. The nature—long or short—of the Beth Shemesh abecedary is unclear (see Sass 1991b: 317), and the tablet is neither stratified nor datable by typology.

[197] While held also for instance by Robin (1997b: 85) and possibly Röllig (1998b: 87–88), the West Semitic origin of the *hlḥ* order is far from being generally accepted; most authors continue to regard it as Arabian.

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Still, advocating as I do an early-first-millennium adoption raises the question: How could the long alphabet be adopted in Arabia from the West Semitic script at a time when it was already obsolete in the Levant? An attempt to justify both the length and the order follows:[198]

Length

For some specialists, the fact that the Arabian alphabet is long (and multidirectional) was sufficient to assign its adoption to the fifteenth, fourteenth or thirteenth century, i.e., to the time preceding the reduction of the West Semitic prototype alphabet. This may be an oversimplification—dating the transmission depends on more than just the number of the letters in the mother script. An early-first-millennium adoption of the alphabet in Arabia is supported and a late-second-millennium one ruled out by West Semitic epigraphical comparisons and the geo-political situation in Arabia and the Ancient Near East (above). These factors must be taken into account in any reference to the length of the Arabian alphabet.

It is therefore possible that we have been asking the wrong question: Whereas at the time of the Arabian adoption the 5–7 graphemes distinguishing the long from the short alphabet were indeed missing from the West Semitic script, the corresponding phonemes in several West Semitic languages were not.[199] Thus, the question is not why the Arabians—and the Greeks—took the sensible step of devising necessary graphemes missing from the prototype alphabet (cf. Sass 1985: 217 = 1991a: 88–89), but rather why the Aramaeans, Hebrews, etc. did not. A by-product of this notion (if its potentiality is accepted) may be a homing-in on the locale—an indication that the Arabians did not adopt the alphabet from the Phoenicians.[200]

Order

The memory of the long letter order (*hlḥm* and possibly also *ʾbgḫ*) could thus have remained with the inland peoples of the Levant and been retained as late as the beginning of the first millennium—even if, with no graphemes for 5–7 phonemes, the exact role this order played then is unknown. Unsatisfactory though it may be, it seems to me that waiting for new discoveries to clarify this role is the only possible course; in light of the very strong evidence favouring an early-first-millennium adoption of the Arabian alphabet, any alternative would be more difficult to defend.

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198 I am no longer satisfied with my old suggestion (1991b: 319).

199 The disappearance of the 5–7 letters is thought to reflect changes in the Phoenician language (*PPG*: 9–11). That Hebrew retained the differentiation between /ʿ/ and /ġ/, /ḥ/ and /ḫ/, is clear from the Septuagint transcriptions (cf. Rainey 2001: 179–181). For Aramaic, the richer repertoire is indicated by orthographic changes (cf. Hug 1993: 47–53). For the Transjordanian languages see, for instance, Parker 2002: 47.

200 In my previous work (1991a: 86–88), due to the 'traditional' chronology, I considered the West Semitic prototype to be specifically Phoenician.

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2.2.2.3 The early South Arabian cursive and the South and North Arabian stone inscriptions

The South Arabian cursive

The best researched representative of the cursive script is the 'minuscule' writing on palm-stalks.[201] The most-archaic-looking examples of this script are currently considered to date to the seventh century (Ryckmans 2001: 224). I hope Ryckmans was right on the dating, though I have not followed all his arguments yet. It should be noted that North Arabian inscriptions of the sixth century and later display several of the peculiarities of the earliest 'minuscule' script (see further below, indirectly also Ryckmans 1997: note 15). ^{14}C analyses of the stalks, currently in the pipeline (Michael Macdonald, pers. comm. Sept. 2003 and April 2004), will constitute a concrete step forward when published. Ryckmans (2001: note 14 and pp. 226–227) observed the similarity between the most-archaic-looking examples of this script and the Yalâ pottery inscriptions (on the uncertainty this adds at present to the dating of Yalâ see p. 111).

This resemblance pertains to the letter shapes, but not to the contents: the Yalâ sherds contain brief texts, mostly personal names, whereas the palm-stalk texts are long and varied—they include abecedaries, lists of personal names, commercial and private letters and letters of recommendation, and contracts (Abd Allâh and Gajda 1997: 117). All this testifies to the spread of literacy beyond official administrative circles. But were the palm-stalks the only medium on which everyday business was recorded in southern Arabia? This is hardly conceivable, for most of these texts seem to have been written by private persons. Yet if official or governmental records, and literary works, presumably pen-and-ink texts written on papyrus or leather or a local substitute, were in use, would not some of them have been preserved in the desert-fringe sites? As far as I know the question remains open.

The growing documentation in recent years of the early South Arabian cursive increasingly demonstrates the links of this script style with the 'pre-classical' South Arabian monumental (Pirenne's Stage A) on the one hand and the earliest North Arabian inscriptions on the other, in turn leading to new insights into the evolution of the latter two. Recently discovered monumental inscriptions of Stage A (see for instance Fig. 26), and the Raybûn pottery inscriptions written in a quasi-monumental style (Fig. 23), improve the perspective. The relevant letter-shapes (see Table 9) may be grouped into five categories:

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[201] At the time of my earlier work (1991a: 87, 90) publications dealing with the cursive script were just beginning to appear. On the great progress made in the last decade see, for instance, Ryckmans 1997, Weninger 2001 and the overviews cited in the main text.

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1. Stage A shapes that, unless I am mistaken, disappear later from all Arabian scripts, north and south, cursive and monumental, like the Sawdâ *mim*.
2. Stage A shapes that disappear later from the South Arabian monumental and cursive scripts but are retained in the north, like the Raybûn *alif*.[202]
3. Stage A shapes that disappear in the later monumental South Arabian script, but are retained in the cursive, as well as in the North Arabian scripts, like the *ṣad* in Ḥarâshif 3.
4. Northern shapes that cannot be shown to have had early southern counterparts. For instance, I could not discover in the south an 'outlined' or double-line *ǧim*. (But the execution of the Sawdâ' *ra* is similar.)
5. Slight graphic variants of the cursive, absent altogether among the better monumental inscriptions but present in certain North Arabian scripts, like the rounded or double-angled *lam*s in Yalâ.[203]

The 'Stage A' and archaic 'minuscule' columns in Table 9 contain South Arabian letter shapes that differ from their 'classical' counterparts in the left-hand column. The right-hand column contains archaic letter shapes of the various North Arabian scripts (cf. Macdonald 2000: 32–34) that are related to the southern 'Stage A' and/or the archaic 'minuscule' writing. Only letters whose shapes are clear in the published photographs and their identification reasonably certain were reproduced. For North Arabian the dating is mostly far from certain.

The links between the cursive and monumental South Arabian are addressed below, followed by a discussion of the links between the southern cursive and North Arabian.

The South Arabian cursive and monumental scripts

The South Arabian lapidary style was definitely not inspired by West Semitic monumental writing; even the best early examples of the latter, like Kulamuwa, display a much less polished ductus. Indeed, the West Semitic scripts had no true lapidary style at all at the time (see p. 58). If a foreign model is insisted upon,

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[202] This *alif* figures however in the South Arabian rock graffiti, of uncertain date to be sure—see Jamme 1963: 43 (it was François Bron who reminded me of this reference). In the Arabian legends on Mesopotamian seals the Raybûn-type *alif* predominates. The absence of this type from the palm-stalk inscriptions (even the most archaic of which have an *alif* similar to the monumental South Arabian letter) may in turn imply that the Raybûn pottery inscriptions are earlier than the earliest palm-stalks known so far. The absence of a similar *alif* from the monumental South Arabian inscriptions of Pirenne's Stage A may therefore be due to either the relative paucity of inscriptions from that early stage or the seniority of the Raybûn inscriptions (on latter see p. 117).

[203] These variants seem to have changed little since the adoption, regardless of the dating of the Yalâ Level A inscriptions (cf. p. 121 with Table 8).

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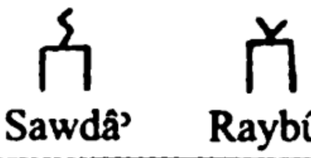
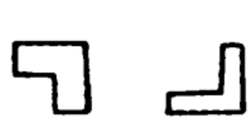
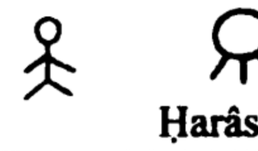
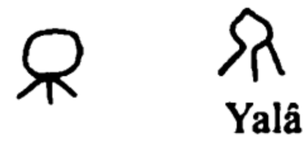
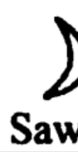
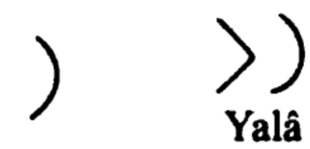
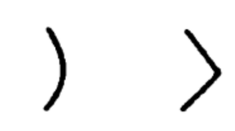
TABLE 9. EARLY LINKS BETWEEN THE SOUTH AND NORTH ARABIAN SCRPTS

	'Classical' monumental South Arabian	*'Stage A' monumental South Arabian, and Raybûn*	*Archaic 'minuscule' South Arabian, and Yalâ*	*North Arabian*
ʾ		 Sawdâʾ Raybûn		
g		Kamna 22		
d		Sawdâʾ Raybûn		
h				
ḥ		Raybûn	Yalâ	
ḫ		Kamna 18	Yalâ	
m		Sawdâʾ	Yalâ	
ṣ		 Ḥarâshif 3	 Yalâ	
r		 Sawdâʾ	 Yalâ	

it would therefore have been indirect—the non-alphabetic stone inscriptions of Assyria, Babylonia, or Egypt. But the execution is decidedly South Arabian, and if so perhaps the inspiration was local, too: The perfect shape of the 'classical' monumental inscriptions is not at all surprising in light of the uniquely geometric and grandiose lines of the earliest monumental South Arabian buildings and art. And it need not lead to the assumption of a lengthy formative period either; are some of the 'pre-classical' Stage A inscriptions, like Kamna 18 (Robin 1992: pl. 55), not already excellence itself?[204] As demonstrated repeatedly in world history, the combination of willpower and money can work wonders in no time.

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[204] And even in Ethiopia, as shown by the monumental inscriptions belonging to Stage A from Yeha and its vicinity (Robin 1998: 797, and passim; I owe the reference to Christian Robin).

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It is due to the nature of the Arabian epigraphic finds—the infinitely greater visibility of the monumental inscriptions, so prominent and so perfect—that the lapidary style until not very long ago had come to stand for 'South Arabian', even though it constitutes only one of the facets of this script. (As is well known, the less careful stone inscriptions and rock graffiti contain cursive shapes to varying degrees.) Yet in principle the existence right from the beginning of an everyday script was clear even before the first cursive examples on palm-stalks were identified as that script.

The differences between the 'classical' South Arabian monumental script and the early cursive script largely reflect the deliberate, hence abrupt, changes made to the former at a time when the same pre-classical shapes in the cursive evolved 'naturally', or much more gradually. From that time on the monumental style will have begun its independent evolution, though some reciprocity with the cursive is to be expected—for instance the *alif* of the palm-stalks, mentioned in note 202.

The South Arabian cursive (and monumental 'pre-classical') and the North Arabian scripts

What data is there for the earliest alphabetic writing in northern Arabia? To begin with, next to nothing is known about the tenth and ninth centuries in that region (and very little about the eighth and seventh centuries), not just as far as writing is concerned; the entire archaeological picture is largely blank. Much more is known about the south. With its different resources and climate and larger sedentary population this territory obviously had greater potential for developed political structures, and it seems to have produced monumental inscriptions long before the north. But it stands to reason that from the beginning the north had had its share in the riches afforded by international commerce and was no less exposed to West Semitic writing.[205] I believe that there is no inherent reason why writing in the north should have lagged behind.[206]

Whereas the earliest securely-dated inscriptions from northern Arabia are those that refer to Nabonidus,[207] Mesopotamian seals with Arabian inscriptions constitute evidence for eighth-century writing in the north (pp. 113–114). In fact,

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205 "[T]he North Arabian script was well established in the seventh century, and probably as early as the eighth, if not the ninth" (Sass 1991a: 89–90).

206 Despite the above (note 205) I concluded in my earlier study (1991a: 89) that the alphabet was adopted in northern Arabia 100–300 years after it was in the south: "The alphabet seems to have been learnt by the South Arabians directly from the Phoenicians, bypassing North Arabia." In retrospect this presumed bypassing and a tri- or bicentennial gap are neither supported nor required by the evidence, and the remaining 100-year gap is a phantom conjured up by the 'traditional' chronology. On the rejection of a Phoenician origin see note 200 herein.

207 First mention and photographs in Eskoubi 1999: 237–239, 251, Pls. 5: 169, 6: 177, and p. 4 of English summary. Then Hayajneh 2001; Müller and Said 2001.

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these seal legends are written predominantly in a style that was considered North Arabian until quite recently.[208] But could some of the examples be pre-standardization South Arabian instead? Or early 'pan-Arabian'?

That the earliest-known North Arabian scripts differ from the 'classical' South Arabian monumental script to the extent they do may, as noted, stem chiefly from the deliberate changes made to the latter at the transition to that exalted stage. Even though rather early, the said changes must therefore date to a time when the north already had its own alphabet.[209] Indeed, the similarities between the monumental 'pre-classical' South Arabian inscriptions and the much later stone inscriptions from northern Arabia seem to indicate that the alphabet was adopted in the whole of Arabia at practically the same time. This is the same conclusion, if for somewhat different reasons, as Robin's (1996a: 1211–1212; 2003: 778).[210] In stone inscriptions (others are barely known) of the sixth century on, the north thus adhered much longer to some of the letter-shapes as originally adopted circa three centuries earlier.[211] What belongs to the interval in the north? Probably several of the seal legends, possibly certain stone inscriptions, long-known but not securely datable, and texts hidden in the unexcavated early-first-millennium settlement strata at the main oases of northern Saudi Arabia.

2.3 THE ANTIQUITY OF THE ARABIAN ALPHABET—SYNOPSIS

The following developments of the last decade and a half may be noted:

- Most important, the slow accumulation of inscriptions from stratified contexts in excavated Yemeni sites—a kind of evidence known until not very long ago only from Hajar Bin Humayd.
- The discovery and publication of the South Arabian cursive, and the contribution of the ensuing comprehension of its relationship with the local monumental styles to a better understanding of the scripts of southern and northern Arabia.

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[208] When the palm-stalk texts began to be published. For the earlier situation see Sass 1991a: 74, and column 2 in the chart on pp. 75–76, the chart subsequently reproduced in Bron 1995: 88. See also Macdonald 2000: 43.

[209] In my earlier work (1985: 218 = 1991a: 90, elaborating on von Wissmann), with hardly any cursive material available yet, I sensed this but had no evidence with which to substantiate it: "Interaction between the North Arabian script and the almost entirely unknown South Arabian cursive script…is likely to have existed, and as long as the latter remains so obscure, scholars can safely blame it for any 'deviation from the norm' in the lapidary inscriptions."

[210] In the latter paper Robin wrote: "From this [the derivation of Dedanite *ẓa* from *ṭa* but of Sabaean *ẓa* from *ṣad*] it can be deduced that the Dedanite and Sabaean alphabets did not originally contain a *ẓ*.... This constitutes a first argument in favour of two parallel developments from the same model...."

[211] Like the cursive of the south.

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- The redating of the West Semitic inscriptions of late Iron I and early Iron II, and the corresponding redating of the adoption of the alphabet in southern Arabia to the first half of the ninth century (or at most *ca.* 950–800 BCE).

To these are added the general South Arabian and Near Eastern picture and their relevance for the emergence of literacy in Arabia.

First and foremost a note of caution is in order. Archaeological excavations by many simultaneous expeditions, unlike the documentation of inscriptions cut in stone, are relatively new in the Republic of Yemen, and furthermore it may take years until a find is published. I am convinced that early Arabia still holds many surprises in store and that the last word has not yet been said about the adoption of the Arabian alphabet as well. In theory a discovery may be made that will illuminate the present evidence in a different light altogether and require a redrawing of the entire picture. But for now the said evidence leads to the following four main points:

The West Semitic prototype of the Arabian alphabet and the date of the adoption

The date of the prototypal Levantine letters is my starting point—the West Semitic shapes closest to the earliest Arabian letters. With a wide margin of uncertainty the range of dates becomes *ca.* 950–800 BCE, and a pre-tenth-century adoption is resolutely excluded in any case by the same evidence. But most probably the West Semitic parallels belong to the transition from Proto-Canaanite to the next phase of the alphabet, *ca.* 900–850, with the mid ninth century my preference.

Concerning the consonantal repertoire, the languages of Arabia had more in common with Aramaic and Hebrew and other West Semitic inland tongues than with Phoenician, and this may constitute a hint for the locale of the adoption.

The earliest Arabian texts from excavations in Yemen and on Near Eastern seals

The dating of the oldest pottery inscriptions from Yalâ and Raybûn, and indeed the archaeological picture of southern Arabia at large, has not yet sufficiently stabilized. A date in the second millennium for the earliest pottery inscriptions from these sites appears to me to be ruled out by the evidence published thus far. The same evidence may point in a general manner to the early first millennium, though not yet to a specific century. Of an archaic ductus close to the oldest inscriptions from Arabia itself are the letters of the Arabian legends on about a dozen Mesopotamian and Levantine seals, in turn dated by their style to the eighth–seventh centuries, and in one case the ninth–eighth. If the seal inscriptions appear to favour a dating of about 800 BCE for the genesis of the Arabian alphabet, the shapes of the prototypal West Semitic letters, as noted, belong rather to the first half of the ninth century. The mid-ninth century therefore emerges as a middle course, and the still largely initial state of Yemeni archaeology leaves the door open for discoveries which may one day point in this direction. Ultimately the question of the time and circumstances of the

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adoption will be settled by better focused ^{14}C datings and credible Palestinian pottery synchronisms of the earliest stratified levels with inscribed finds at Yalâ, Raybûn and contemporary sites, including a re-evaluation of the all-important old excavation at Hajar Bin Humayd.

The general South Arabian and Near Eastern picture

My underlying assumption for the adoption of the alphabet in Arabia is that this achievement cannot be separated from the Arabian incense boom or from the general Fertile Crescent picture. It was the domestication of the dromedary in the late second millennium that made a quantum leap of the long-distance overland trade potentially possible. But at both the Arabian and the Canaanite ends of the route there are no Late Bronze Age indications for so early a realization; the domestication probably came too late for that. Nor, to all appearances, did the Levant in the earlier phases of Iron I, *ca.* 1150–1000/950 BCE, constitute an overly lucrative market. The realization of the said potential could only have begun towards the end of Iron I, about the second half of the tenth century, when the southern Levant witnessed a final revival of the Canaanite culture. And with the rise of the Assyrian empire and the emergence of territorial states in the Levant about 900 BCE, a veritable boom in Arabian trade certainly came to pass.

The growing economic power of the Arabians, the interaction with the literate societies to the north, the need for a writing system and its fulfilment—all are thus phenomena of the tenth century at the very earliest, but more probably of the ninth. The first states, and monumental architecture, art and inscriptions, soon followed in ancient Yemen: Rich and powerful as the Arabians became in the ninth century, there is no reason why it should have taken them another full century to imitate—and in several respects surpass—their northern customers. This is why I believe that when conclusive chronological indications for the beginning of South Arabian monumentality are ultimately discovered, they will point not to the eighth century still preferred by most specialists, but to the ninth. This is also why I take the South Arabian monumental script to be only slightly younger than, if not essentially contemporary with, the adoption of the Arabian alphabet itself.

The North Arabian alphabets and their southern connections—monumental and cursive

The earliest phase of the alphabet in northern Arabia is still practically unknown. All the same seal legends, the available trickle of reasonably well-dated excavated inscriptions from the south, and the cursive inscriptions on palm-stalks likewise from the south may permit the outlining of a picture, however preliminary or tentative. The earliest, 'pre-classical' monumental South Arabian inscriptions contain letter shapes that disappear from the subsequent standardized, monumental inscriptions. Some of these shapes were known otherwise chiefly in the North Arabian scripts centuries later, but appear now to have been retained in the South Arabian cursive

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even after they were removed from the monumental script. All this may indicate a single adoption of the alphabet in Arabia in the ninth century, and the subsequent independent development of the scripts in the south and the north.

Summary of the chronology

The ninth century is the most likely time for the adoption of the alphabet in Arabia and—with no need for a lengthy formative period as I see it—also for the emergence of monumental inscriptions (see addenda, p. 184). But this has still to be confirmed by the results of excavations containing inscriptions. If my dating of the West Semitic alphabetic prototype about the middle of the ninth century is valid, the radiocarbon datings for the earliest stratified inscriptions in southern Arabia, once settled, will cluster around that time and the Palestinian pottery parallels will fall in early Iron II. As to the North Arabian oases, not only were the conditions there very different from the south, but the circumstances under which the alphabet took root in places like Tayma and al-Ula are wholly unknown. Yet according to the earliest letter shapes the adoption of the alphabet in northern Arabia must have been part of the same development and taken place at the same time.

* * *

At the close of this chapter I wish to share with the readers a 'what if' reflection: What would Jacqueline Pirenne's work have looked like had she had at her disposal the present evidence about the cursive script of the palm-stalks, Yalâ sherds, etc.?

It was perhaps chiefly the absence of early-first-millennium South Arabian cursive inscriptions that led Pirenne astray. Inevitably she (like everyone else) had been over-dependent on the monumental South Arabian script. This prevented her from evaluating correctly the conservatism of the North Arabian script of the sixth–fifth centuries, which retains 'Pan-Arabian' alphabetic features some 300 years older, in turn linked to the Stage A monumental South Arabian script. Instead, Pirenne took the said North Arabian ductus to represent a pre-lapidary beginning of southern origin, even while conceding the eighth-century date of the lapidary, refined seal inscriptions (1955: 19, 41–42; 1956: 99–101).[212] Pirenne's Greek theory ultimately compelled her to close her eyes to the ensuing contradiction. The following quotation seems to me to summarize Pirenne's attempt to handle the problem (1955: 19; my thanks to Michael Macdonald for his help with the translation):

> Albright noted that the Dedanite, and with it certain stamp and cylinder seals…, display a writing which…seems to present, it is true, an archaic stage of the Minaeo–Sabaean script. But what should we make of the dates? Even if certain cylinder seals could be dated to the 7th century and if one of the seals could, at

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[212] See Sass 1991a: 84, note 31: "[A]s for Pirenne's stand,…the 'Proto-Arabic' seal inscriptions are sufficient to refute her own low chronology." And compare note 184 herein.

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a pinch, go back to the eighth, the fact remains that at this date, according to the accepted [i.e., pre-Pirenne] theory, the Sabaean script would already have been monumental and of the most accomplished style. The facts which might suggest replacing the South-to-North movement [of the script] by one which went from North to South, conflict with the dates, which form a stumbling-block to all [attempts to pursue] research in this direction.

One cannot be certain that had Pirenne lived to behold the most recent developments in the study of the South Arabian cursive, she would have at such a late stage in her life conceded her fundamental error. But had she had the same evidence at her disposal at the beginning of her career, it is conceivable that the notion of the Greek connection and of so low a dating would never have occurred to her, and that as a result her absolute chronology would have been as ground-breaking as her relative chronology.

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CHAPTER 3

THE ANTIQUITY OF THE GREEK AND PHRYGIAN ALPHABETS

3.1 INTRODUCTION

This chapter addresses new evidence and insights concerning the genesis of the Greek alphabet that have necessitated a revision of my earlier views (cf. Sass 1985: 235–237 = 1991a: 94–98).[213] The chapter also deals with the potential precedence of the Phrygian alphabet, an issue I did not address at all in my previous work.

In one respect this chapter is directed in particular at classicists: I believe that their misgivings concerning Semitic epigraphy, while justified, pertain not to the evidence (which to my mind is irreproachable), but only to the 'traditional' handling of it, and I have done my best to outline this in a manner understandable to non-Semitists as well.

A word on the absolute chronology of West Semitic epigraphy employed in this book (in contradistinction to the 'traditional' chronology on the interval 1150–850 BCE only). Even though it was my West Semitic chronology that led me to rethink the genesis of the Greek alphabet, this chronology has no direct bearing on the present chapter, which is about the period after 850. At first sight what could constitute a potential link are Naveh's late Proto-Canaanite parallels for the earliest Greek letters: According to my West Semitic chronology they would belong *ca.* 900–850 BCE rather than 1100, reducing to just a few decades the gap between Naveh and his adversaries. Yet the very connection between Proto-Canaanite and the earliest Greek letter-forms is contested (below).

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[213] Quite a few works have lately dealt with the genesis of the Greek alphabet; the present chapter also seeks to complement some of these, by Semitists and classicists, e.g., Amadasi Guzzo 2000 and Whitley 2001: 128–133.

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3.2 THE ADOPTION OF THE ALPHABET BY THE GREEKS

3.2.1 THE ANTIQUITY OF THE GREEK ALPHABET—SELECTED STUDIES 1933–2000

Pre-1930s authors mostly dated the Greek alphabet, on the flimsiest evidence, to various centuries of the second millennium BCE. Then, in 1933, Rhys Carpenter published his seminal article on the subject.

Carpenter's point of departure, that "the Greek alphabet must have branched off from the Semitic at the point where...the two sets of alphabets most nearly agree in the forms of letters" (1933: 10) also served the majority of subsequent studies of the genesis of the Cadmean letters. Accordingly Carpenter dated the adoption first (1933) to the end of the eighth century, then (1938) to the 720s, and finally (1963) to the middle of the eighth century. The last is close to the time to which the Phoenician 'Baal Lebanon' inscription is commonly assigned (*KAI* 31, *TSSI* 17). Indeed, the 'Baal Lebanon' letters—shapes and vertical stances—were long recognized as resembling the oldest Greek letters most. This becomes abundantly evident when that particular inscription is placed side by side with the Greek inscription on the Dipylon jug of the second half of the eighth century (Figs. 27–28). See Addenda, p. 184.

In its day, Carpenter's view was met with strong opposition mostly, I am sure, because old habits die hard. But gradually Carpenter's manifest logic prevailed. In the decades following World War II, his view was almost unanimously accepted by classicists, and by no few Semitists (cf. McCarter 1975: 22–27). New discoveries led classicists eventually to raise the date to *ca.* 750 BCE,[214] while Semitists came to favour the late ninth or early eighth century. Then, in 1973, Joseph Naveh rattled the field of Hellenic studies (much less Semitic studies) with his thesis of a late-second-millennium adoption of the Greek alphabet. He founded his argument on the similarities between several archaic Greek and late Proto-Canaanite letter-shapes. The latter he assigned *ca.* 1100 BCE according to the 'traditional' chronology of West Semitic epigraphy.

That the Greek evidence was in utter disagreement with his scheme did not deter Naveh, nor was his conviction shaken by the elaborate apologetics required to justify the three-century gap which, in line with his chronology, separated the adoption of the alphabet from the earliest Greek inscriptions.[215] An answer to the question of what led him to accord this evidence such low priority is perhaps to be found in

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[214] As noted, Carpenter himself had eventually (1963: 77) come to that opinion: "most probably the third quarter of the eighth century."

[215] Some of the Greek evidence contradicting his thesis was referred to by Naveh himself (see p. 143).

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Fig, 27. 'Baal Lebanon' bowl inscription

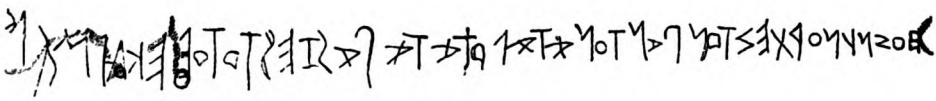

Fig. 28. Athens, Dipylon jug inscription.

Naveh 1987: 102: "The reconstructions that Cross made in 1967 enabled the writer to conjecture in 1973 that the Greeks must have adopted the twenty-two letter alphabet from the Canaanites about 1100 BCE."

In theory Semitists should have been easier to win over, because on their side of the equation ocular comparison of the letter shapes could seem quite convincing, and perhaps also because some of these specialists were not overly concerned with the difficulties Naveh's thesis met with on the Hellenic side. But in reality only a handful of Semitists declared themselves unequivocally behind Naveh, and even this support arrived only after seven years had passed (*ca.* 1980), when adherents of the 'traditional' chronology of West Semitic epigraphy found themselves fighting back attempts to date the Tekke bowl *ca.* 900 BCE against their preferred 1100. (Though still unpublished, the Fekheryeh statue was already known at the time.) Naveh's thesis was then embraced not so much for its own sake as for the support it was thought to lend to the higher date of the bowl. This 'aging' of the Tekke bowl, in turn, was indispensable for maintaining the more developed script of Ahiram at the 'traditional' high of 1000 BCE (see p. 36 with note 33, and the quotation from Cross in Naveh 1982: 186). Most Semitists, however, found Naveh's idea problematic at least to some degree (summarized in Naveh 1982: 185); others held it to be inadmissible (details below).

Classicists, in their turn, were much troubled by the irresolvable conflict between the Greek evidence and Naveh's unquestionable authority in a field with which they were personally less acquainted. Consequently, several of them admitted to be at

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a loss about how to settle the question.[216] Other classicists resisted, emphasizing the real problems on the Greek side, at times adding seeming problems of Semitic palaeography meant to discredit this field as a whole (details on pp. 151–152).

However, for straightforward Hellenic as well as Semitic considerations, all known before 1973, a dating *ca.* 1100 BCE for the birth of the Greek alphabet was in fact never an option at all. Three aspects are at play here:

1. The features in the early Greek inscriptions that Naveh took to be Proto-Canaanite.
2. Naveh's notion of an initial Greek 300-year inscriptional void and palaeographical 'standstill'.
3. Naveh's concept of a multiple-stage adoption.

A fourth aspect, the 'monumentality' or verticality of the adopted Greek letters, remains valid for an eighth-century adoption (below).

Proto-Canaanite features?

The eighth–sixth-century Greek features reminiscent of the Proto-Canaanite script alone are actually misleading. Not Semitic in the least, these features—multidirectional writing and the fluid stances of individual letters, as well as legless *mu* and dotted *omicron*—were always deemed local post-transmission developments by the classicists, with good reason, I believe.

On the former see Jeffery 1984: 377*:* "[L]ines *consistently* [italics Jeffery] retrograde, like in Phoenician, are very rare: their [the Greeks'] earliest inscriptions, once beyond the first line, maeander back 'ox-plough-wise'. Then, during the sixth century, they settled into continuous left-to-right...." (An alternative source of boustrophedon writing is mentioned in note 242.) Also Amadasi Guzzo 1991: 305:

> [T]he stance of certain signs (*alpha*..., *lambda*..., *sigma*...)...may be explained not by contemporaneity and the dependence of one script on the other, but rather by the analogous situation of the alphabet in the respective regions and periods. The late Canaanite inscriptions as well as the first Greek ones seem to originate from periods and places devoid of a solid scribal tradition and a standardized canon.[217]

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[216] E.g., Whitley 1992: 124: "[T]he old 'letter form' problem...is the observation that some earlier Phoenician letter forms (of *c.* 1200–1100 BC) resemble Archaic Greek letters more closely than they do Phoenician letters current *c.* 800 BC. The 'letter form' problem...has...been employed by...Joseph Naveh to update the Greek adoption of Phoenician script. It deserves more serious and extensive treatment than it receives here."

[217] On the stances of individual letters see further Sass 1985: 235 = 1991a: 94–95 and note 34 (still according to the 'traditional' chronology): "It is preferable not to base the argument for an early borrowing of the Greek alphabet on letter stances and multidirectional writing. How does one distinguish between supposed eleventh-century Proto-Canaanite/Phoenician influence and later, independent Greek developments?...The vertical stance of *sigma* invoked by Naveh does not strengthen his position—did not *lambda*, for instance, also rotate without any eleventh-century Semitic parallel?"

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The legless *mu* and dotted *omicron*, in turn, are later variants of the transmitted legged *mu* and dotless *omicron*—see Table 10 on p. 141. Jeffery (1982: note 8) remarked that "[Naveh's] argument is wrong assuming that the tailless forms of *mu* and *psi* are early." Johnston (1990: 426–427) shared her critique:

> [I]nsufficient regard has been paid to the earliest Greek texts when letter-forms are compared. Naveh cites [the five-stroke legless *mu*] and dotted *omicron* as 'early' borrowings, i.e., before *ca.* 1000; yet neither is found in the earlier Greek material; they are both, in different ways, developments of the received shapes.[218]

Similarly Amadasi Guzzo (2000: 236–237): "Regarding the forms of the signs, researchers in the domain of Greek epigraphy remarked that the analyses of J. Naveh did not take into account the relative and absolute chronology of the relevant archaic Greek inscriptions."

Many among the twenty-two received forms from *alpha* to *tau* are chronologically neutral (i.e., long-lived), while others distinctly corroborate a late-ninth–eighth-century dating for the adoption (see pp. 140–143). In sum, the Semitic prototype was definitely post-Proto-Canaanite, and the conclusion that the birth-date of the Greek alphabet either equals the time of the earliest extant Greek inscriptions *ca.* 750 or slightly precedes it fits both the Greek and Semitic evidence.[219] As an eighth-century development, moreover, the adoption of the alphabet may very aptly be regarded as part of the Orientalizing phenomenon (e.g., Burkert 1992: 25).

Tercentennial inscriptional void and multiple-stage adoption?

When dealing previously with this question (1985: 236 = 1991a: 98) I found it untenable that the Greeks should have adopted the alphabet three centuries before their first inscriptions appeared.[220] Yet my proposed dating in the ninth century cannot be maintained. The above reasoning of Jeffery, Amadasi Guzzo and others in favour of an updated version of Carpenter's eighth-century thesis—and excluding the Proto-Canaanite link entirely—which I did not consider in 1985/1991, I found preferable to my own as soon as it came to my notice.[221] It is followed from this point on.

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[218] On the dotted *omicron* see further Amadasi Guzzo 1991: 304: "[T]he *omicron* with central dot is not, in all likelihood, derived from the Phoenician (or Canaanite) model: the dot in fact does not appear in the most ancient Greek documents such as the Dipylon vase and Nestor's cup." Occasionally a dotted circle, chiefly in the inscriptions of Thera and Crete, was identified as a 'proto-*omega*' (Jeffery 1961: 37, 308–309, and the Table of Letters at the end of the book; Johnston 1990: 428; Johnston *apud* Dimoudis and Kyriatsoulis 1998: 462; Bartoněk and Buchner 1995: 178).

[219] Similarly Lemaire (1991: 145–146) and Röllig (1998a: 371), among others. On the several decades' difference between classicists and Semitists see pp. 145–146.

[220] Jeffery's reaction (1982: note 8) was remarkably mild: "[Cf.] Naveh [1973], an article deserving serious consideration by Greek epigraphists, though the blank on the Greek side before the eighth century remains a problem...." But see the quotation in the main text.

[221] In my 1991 book (p. 3) I still labelled the option of an eighth-century adoption "impossible".

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Jeffery's and Amadasi Guzzo's rationales for the single-stage adoption and against the multiple-stage one are particularly well argued,[222] though with the Proto-Canaanite option gone, there is hardly need to defend the former at all.

A monumental source

The verticality of many of its early letters led Naveh (1973: 3) to the view, already held by Carpenter (1938: 67), that the Greek alphabet should be traced back to a lapidary (or monumental) West Semitic source. Let me add that such a source may indirectly substantiate the eighth-century dating of the adoption: Ninth-century forms lingered into the following century in Phoenician–Aramaic monumental inscriptions and other texts incised on hard surfaces, which as a rule were more conservative than ink inscriptions. Verticality also characterizes the Phrygian letters.[223]

3.2.2 THE MOTHER SCRIPT AND LOCALE OF THE TRANSMISSION—SELECTED STUDIES

The attempts to locate the birth-place and identify the mother script of the Greek alphabet are necessarily interdependent—a Phoenician derivation of the Cadmean letters would imply a range of potential locales different from a Phoenician–Aramaic derivation, etc.

Greek tradition labelled the source 'Phoenician', but as Jeffery noted (1982: 833), this may have referred to the West Semitic realm at large. Indeed, it has long been held by some commentators that Aramaic influence on the Greek alphabet (in addition to Phoenician) is apparent in the *-a* endings of many Greek letter names, and even more so in the vowel letters, presumed to have their origin in the *matres lectionis*. That the Phoenician and Aramaic scripts were at the time of the adoption still nearly indistinguishable in monumental inscriptions (see note 87) may constitute another point in favour.

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[222] Cf. Jeffery's critique of the authors who saw indications for multiple-phase adoption in the same shapes and phenomena of the archaic Greek script which she regarded as post-transmission diversification (Addenda and corrigenda, 1961: 374): "[A]ll the varieties had certain common characteristics (the five vowels, the style of the letters, the alteration of certain consonantal values, and the invention of new letters).... [I]t is suggested that a nucleus of bilingual Greeks in Al Mina, or in that general area, adopted the Semitic script, producing in their alphabet the characteristics noted above, and that the variation in shape of certain letters...came chiefly...in the transmission thence to other Greek centres." Amadasi Guzzo 1991: 305: "It seems to me that the hypothesis of a secondary adoption, still from the Phoenician alphabet, of *kappa* and *upsilon* is to be rejected...: once a system is established and functions for a certain time, it seems rather difficult that one should fall back on a foreign source not for adopting a new system, but for modifying some details – in this case the forms of certain signs – that were already present in the original model."

[223] This may somewhat weaken Johnston's view (1990: 426; 1992: 122) of the same phenomenon in the Greek script as reflecting 'geometricization', a local development (or rather enhancement) in accordance with the style of the period. See further p. 144 and Table 12.

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The reasoning of other authors against Aramaic inspiration (i.e., in favour of exclusively Phoenician inspiration) is well known too; several of the arguments for and against are listed in Naveh 1973: note 32. More recently Lemaire (1991: 144–145) observed the limited use of *matres lectionis* also in Phoenician—in the writing of Luwian names in Cilicia,[224] and the potential impact of such a practice on the creation of vowel letters in the Indo-European alphabets.[225]

Assuming for the next few paragraphs that the Greek alphabet preceded the Phrygian, the source of the former would be Phoenician or Phoenician–Aramaic, with diverse possible locales;[226] a definite choice does not present itself. (This has been stressed time and again by authors concerned with the general picture, for instance Amadasi Guzzo 1991: 308–309, quoting Coldstream.) Two scenarios for the mother alphabet(s) and the locale are therefore in order.

1. *Exclusively Phoenician.* If this option is preferred, the transmission could have occurred practically anywhere on the Mediterranean shores where Greeks and Phoenicians met. In this regard ninth- and eighth-century Phoenician inscriptions from the West (see p. 68) remind us that not only did the Greeks pursue the Semitic alphabet; it pursued them as well.
2. *Phoenician–Aramaic*. The area where the transmission occurred may be narrowed down considerably if the locale of this development is assumed to have been a bilingual or even trilingual milieu on the Levantine coast,[227] in a region where Greeks could have come into contact with both the Aramaic and the Phoenician scripts, say between the Amuq and Cilicia, or between the former and Arwad.

Another theoretical possibility raised in the past, the adoption of the alphabet by the Greeks under exclusive inland Aramaean influence, remains unsupported by the evidence: That in central and southern Anatolia the only Semitic alphabet in use before the Persian period was the Phoenician quite probably excludes this (see for instance Winter 1979: 138).[228]

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[224] Also Amadasi Guzzo 1995b: 72; *PPG*: 58. *Matres lectionis* are otherwise absent from Phoenician; they are present in the Aramaic script since its beginning in the ninth century (see pp. 51–52).

[225] The notion of an opposite development, i.e., regarding the Greek vowel letters as the source of the *matres lectionis* in the Phoenician inscriptions from Cilicia (Brixhe 1994: 82) is contradicted by the dating of the relevant texts.

[226] Cf. the rather long listing in Amadasi Guzzo 1991: 308, to which may be added Cilicia (Lemaire 1991: 145–146), Euboea (Powell 1997: 22) and Ischia (Holloway 1994: 112 with note 14).

[227] See the quotation from Jeffery in note 222.

[228] The origin of the Greek word dividers in the shape of two or three vertically arranged dots, suggested by Bordreuil (1993: 263) to have been the Fekheryeh inscription, will thus have to be sought elsewhere (and in any case it was certainly not the archaizing Fekheryeh ductus which inspired the first writers of Greek—cf. Naveh 1987: 105–106). The like dividers in Phrygian (Brixhe and Lejeune 1984: 279) will have had another source too, though the Phrygian and Greek dividers could be related to each other.

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And if the Phrygian alphabet preceded the Greek or if both were adopted simultaneously, the Semitic source is very likely to have been the Phoenician monuments of Cilicia or Tyana (see pp. 148–149).

* * *

The next sections (and Excursus G) present additions to the above picture. Among them:

- The Semitic evidence indicating a time-range *ca.* 825/800–750 BCE for the transmission, and the grounds for narrowing down this range. It seemed to me necessary to review the material because of the disparity between the present preference among Semitists of a late-ninth-century transmission, and the Greek finds, which for now preclude a date much higher than 750 BCE.
- An attempt to account for the enhanced 'Proto-Canaanite like' look that certain Greek letters acquired after the transmission, in the seventh–sixth centuries.
- Further support for the potential of Phrygian seniority in adopting the alphabet and devising the vowel letters.
- Some thoughts on the cultural and political background of the transmission at both the giving and receiving ends.
- The repercussions of the classicists' bewilderment in the face of Semitic epigraphy.
- The Osteria dell'Osa graffito, and why raising the date of the Greek alphabet should not be founded on it.

3.2.3 THE ANTIQUITY OF THE GREEK ALPHABET

According to their chronological significance, the twenty-two adopted Greek letters fall into three categories:

1. Many among the thirteen letters that bear some resemblance to earlier Semitic writing were believed to be Proto-Canaanite in origin and to date *ca.* 1100 BCE. Yet these are simply long-lived forms (though much less long-lived than the 'traditional' chronology would have it); they did not change a great deal between the late Proto-Canaanite phase and the early Phoenician–Aramaic script of the ninth–eighth centuries (Column 1 in Table 10; cf. Sass 1985: 235–236 = 1991a: 95).
2. The shapes of four other letters (Column 2) are unhelpful for another reason—in the Greek alphabet they seem to have undergone changes immediately upon the transmission.
3. The five remaining Greek letters (Column 3) resemble Semitic shapes which do not occur before the ninth century. Of considerable chronological importance as a

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TABLE 10. THE TWENTY-TWO GREEK LETTERS FROM *ALPHA* TO *TAU*—NATURE OF THEIR SEMITIC DERIVATION

		1. Semitic, long-lived	*2. Changed in transmission?*	*3. Semitic, 9th–8th centuries*	*4. Seemingly Proto-Canaanite*	
1	*alpha*	+				
2	*beta*		+			
3	*gamma*	+				
4	*delta*	+				
5	*epsilon*			+		
6	*digamma*		+			
7	*zeta*	+				
8	*eta*	+				
9	*theta*	+				
10	*iota*		+			
11	*kappa*			+		
12	*lambda*	+				
13	*mu*			+ (legged)		later legless variant
14	*nu*			+		
15	*ksi*	+				
16	*omicron*	+ (dotless)				later dotted variant
17	*pi*	+				
18	*san*		+			
19	*qoppa*	+				
20	*rho*	+				
21	*sigma*	+				
22	*tau*			+		

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terminus post quem for the transmission, three or four of them (save *kappa*, and to some extent legged *mu*) may have hitherto received less attention than they deserve.

Outside the present count (13 + 4 + 5 = 22) are the seemingly Proto-Canaanite but in reality much later variant shapes—the legless *mu* and dotted *omicron* (Column 4 and pp. 136–137). The legged *mu* of the time of the transmission is listed in Column 3, the dotless *omicron* in Column 1.

The following elaborates on the five letters in Column 3:

- Oblique, legged *epsilon* originated in the similar form of Semitic *he*, a form not earlier than the ninth century (see Sass 1985: 235 = 1991a: 95, and p. 40 herein). *Epsilon* later becomes right-angled and legless (see below).
- *Kappa* is evidently a 'verticalized' version, its leg often truncated, of the mainstream oblique Phoenician–Aramaic *kap*, beginning in the ninth century, its long stroke or leg a continuation of the right-hand finger, not of the middle finger (see Sass 1985: 236 = 1991a: 96, and p. 43 herein).
- The earliest *tau*, occasionally still †-shaped rather than T-shaped, is not too far removed from Semitic scalene (i.e., legged) *taw*, which emerged in the ninth century. The Semitic form in the eighth-century 'Baal Lebanon' inscription is particularly close to its Greek counterpart; it is nearly vertical whereas *taw* is habitually oblique (see pp. 32–33).[229]

These ninth-century Phoenician–Aramaic forms lived on, so that they may likewise be cited in support of an eighth-century adoption. One form, or rather two, may moreover belong exclusively to the eighth century:

- The legged *mu*, for instance in the Dipylon jug, had its prototype in the advanced, legged and horizontal-headed *mem*—which likewise occurs in the 'Baal Lebanon' inscription. Most Phoenician–Aramaic *mem*s in securely-dated monumental inscriptions of the second half of the ninth century still display more archaic, oblique heads (see for instance Naveh 1970b: Fig. 1).
- Legged *nu*. By extension the above also applies to this letter.

On the other hand, the Semitic prototype of one Greek letter continues into the eighth century only by exception: The model of the triangular *delta*—one of the latest occurrences of the long-lived, legless *dalet*—is similarly found in the 'Baal Lebanon' inscription. This inscription may be partly archaizing, as befits a text incised in hard material and employing a monumental ductus.

An interesting alternative for the origin of the triangular *delta* was offered by Carpenter (1938: 69, addendum). He considered the loss of leg to be one of the Greek

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[229] My earlier observation (1991a: 95) on the loss of one stroke in *tau* was thus mistaken.

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alterations that occurred practically upon the transmission (though not an alteration so drastic as in *beta* and *iota*, for example).[230] In the case of *delta* he surmised that this was done in order to differentiate it better from *rho*.

Concerning the *he*, *kap*, *mem*, *nun* and *taw* as an ensemble, I should emphasize that they are altogether diagnostic for the dating of the transmission. As shown in Tables 2 and 11 there are hardly any other letters which change much between the late Proto-Canaanite phase and *ca.* 750 BCE,[231] *and* remain recognizable in the early Greek inscriptions. Among the five Greek letters (*epsilon*, *kappa*, *mu*, *nu* and *tau*) Naveh (1973: 7, 8) accepted two—*kappa* and *mu*— as having ninth-century Semitic prototypes:

> [T]he *kappa*, the only evident form which speaks against the assumption that the Greeks borrowed the West-Semitic writing in the late twelfth or early eleventh century B.C.... It would be much simpler to assume that the Greek adoption of the alphabet took place in a bilingual environment of which we have indications only from the late ninth century B.C.... Also the Greek *mu* sign looks much nearer to the corresponding forms of the Semitic inscriptions from about 850 B.C. on, than those of the earlier inscriptions.

As is well known, Naveh chose to justify these two forms by assuming a multiple-phase adoption (see p. 138), and to disregard altogether the contrary message of the other three, or rather of all five combined (for a similar approach to Fekheryeh see note 44).

A remark is in order on how the similarity between some Greek letter-forms and their purported Proto-Canaanite parallels could exist despite the chronological disparity. Viewed schematically, three script phases may be discerned (see Table 12):

1. *A late Proto-Canaanite 'pre-cursivization' phase in the West Semitic alphabet.* The starting point.

2a. *The 'cursivization' of the West Semitic alphabet after* ca. *900 BCE* (pp. 51–52). It involved *inter alia* a slight top left rotation of some of the previously upright letters (*he*, *ḥet*, *yod*, etc.), and a lengthening of one stroke (generally the rightmost) to become a 'leg'—e.g., in *dalet*, *he*, *kap*, *mem*, *nun* and *taw*. This was no doubt the result of the proliferation of fast writing with pen and ink on papyrus or leather. All the same (see note 68) no few of these changes soon surfaced in monumental

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230 "After this article was set up, the Annual Meeting of the Archaeological Institute of America produced several papers germane to the general topic.... Prof. Day's review of the Semitic evidence suggested that my article has not adequately emphasized the error of appealing to the cursive flourish at the end of the pen-stroke which produced the 'tails' on Semitic letters. The Greek *genius scribendi* inevitably eliminated these tails, some immediately, others more gradually (cf. D, E, H, K, M, N, Y). Thus a tailed Semitic D would have produced a curtailed Greek one."

231 Thus already Naveh 1970b: 10 ('traditional' chronology).

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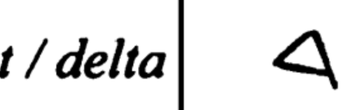
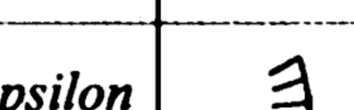
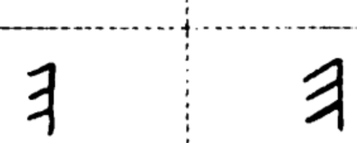
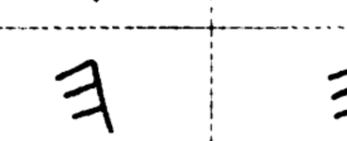
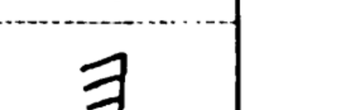
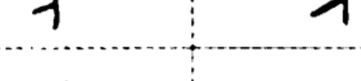
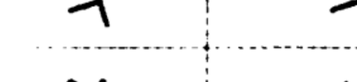
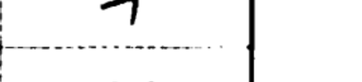
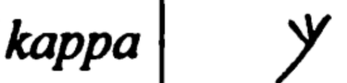
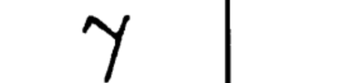
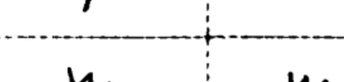
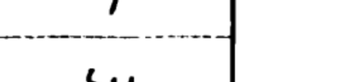
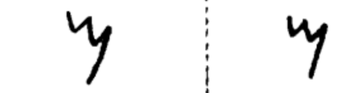
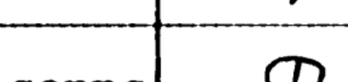
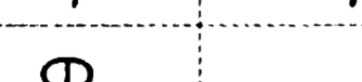
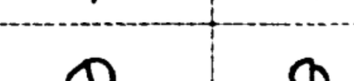
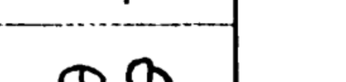
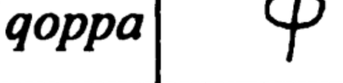
TABLE 11. DIAGNOSTIC EARLY GREEK LETTERS AND THEIR WEST SEMITIC COMPARISONS

	Kulamuwa (ca. 830)	*Earliest Greek*	*Baal Lebanon (ca. 750)*	*Sfireh (ca. 750)*	*Panamuwa/ Barrakab (ca. 730)*	*Karatepe (ca. 700)*
dalet / delta						
he / epsilon						
kap / kappa						
mem / mu						
qop / qoppa						
taw / tau						

inscriptions, themselves probably the model for the earliest Greek or Phrygian script.

2b. *The transmission of some such forms to the Greek alphabet.* A few early Greek forms of *epsilon* and *eta* followed the rotated, or oblique, and acute-angled Semitic forms. (Most other Greek letters either have vertical, right-angled Semitic variants as their prototypes, such as the 'Baal Lebanon' *taw* for the Dipylon *tau*, or else these forms were 'verticalized' at the transmission.) Likewise, several Greek letters—e.g., *epsilon*, *mu*, *nu*—were taken over with their Semitic legs (while the leg in *delta* was possibly eliminated upon the transmission, and in *kappa* truncated—see also note 230).

3. *Greek 'de-cursivization' after the eighth century.* Eventually a reverse process took place in the Greek letters which still retained Semitic cursive traits. The oblique forms were re-verticalized, the acute angles became right again, and the legs were eliminated. These inner Greek developments,[232] no earlier than the seventh–sixth centuries, lent *epsilon*, *mu* (the three- or four-zigzag variant without leg) and *nu* their seeming late Proto-Canaanite look.

It follows that the resemblance of some archaic Greek letter-shapes to Proto-

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[232] As well as the addition of a dot to *omicron* (see p. 137 with note 218).

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TABLE 12. POST-TRANSMISSION MODIFICATION AND ENSUING PROTO-CANAANITE LOOK OF THREE GREEK LETTERS

	Late Proto-Canaanite	*Early Phoenician and Aramaic (9th–8th cent.)*	*Greek adoption (8th cent.)*	*Later Greek 'de-cursivization'*
he / epsilon	E ('Izbet Ṣarṭah)	𐤄 𐤄 𐤄 𐤄	𐌄 𐌄 𐌄	E
mem / mu	𐤌	𐤌 𐤌 𐤌 𐤌	𐌌 𐌌	𐌌 M
nun / nu	𐤍	𐤍 𐤍	𐌍	N

Canaanite forms is a phantom;[233] the Semitic set of letters closest to the earliest Greek letters is definitely that of the ninth or eighth century. This range may be narrowed down:

Bottom end

Its time *ca.* 750 BCE is provided by the earliest Greek inscriptions, in agreement with the date of the Semitic prototypes. After the mid-eighth century several Phoenician and Aramaic letters, for example *kap* and *qop* (Table 11), evolved away from the shapes that served as models for the corresponding Greek letters.

Top end

The advanced Phoenician–Aramaic *he*, *kap* and *taw* may already have existed in the early ninth century (p. 58). Yet a corresponding upward extension to *ca.* 900 BCE of the adoption of the alphabet by the Greeks is ruled out by the horizontal-headed *mem* just mentioned: Very probably post-800 BCE, this form is certainly post-850. An upper limit *ca.* 825 is about as early as possible.

Hence, a range of *ca.* 825/800–750 is reasonably substantiated by the Semitic evidence.[234] Carpenter, with much less Semitic evidence at his disposal, reached a similar conclusion (1938: 66): "[T]he archaic Greek alphabet is unmistakably of the type used in Semitic inscriptions of the late ninth and the eighth centuries B.C.... (i.e., 825–725 B.C)." The top end may one day be shown to apply to the Semitic prototype of the Phrygian alphabet—see pp. 146–149. And as regards the Greek alphabet, it may be due to the time range permitted by the Semitic comparisons that several

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[233] This would remain so even in the unlikely case that someone attempts to reconcile Naveh's thesis with my West Semitic chronology, i.e., suggest an early-ninth-century adoption: Typical in Greek inscriptions of the seventh–sixth century or later, but not earlier, the legless *epsilon* and *mu* and dotted *omicron* are too young in any case to reflect a Proto-Canaanite memory, even one as late as *ca.* 850 BCE.

[234] It supersedes the time span I suggested in 1985/1991.

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Semitists in the last decades favoured the late ninth century or early eighth for the transmission.[235] But such an early setting is precluded by an authoritative piece of negative evidence on the Hellenic side—the absolute absence of Greek inscriptions among the thousands of pots and sherds of the Middle Geometric period, customarily dated *ca.* 850–750 BCE (cf. Carpenter 1938: 62; Jeffery 1961: 17; Burkert 1992: 28). This absence is accentuated further by the rarity of Greek inscriptions on Late Geometric pottery of the second half of the eighth century. Indeed the said evidence (or absence) makes even a safety margin of fifty years upwards from 750 BCE appear excessive.[236] This situation will change only if classicists one day revise upward the initial date of LG (see p. 154), or if securely dated MG inscriptions come to light after all.

A word on the Osteria dell'Osa graffito may be due at this point: Occasionally proclaimed the earliest-known Greek inscription, the graffito is too problematic to found an upward revision of the Greek alphabet on it. See further Excursus G, pp. 155–156.

Looking back, the gist of the above view was in 1985/1991 or 1973 as evident as it is today. In a sense, this returns us to Carpenter 1933/1938 (with a slight upward shift), reaffirmed by the subsequent accumulation of knowledge in the Greek and West Semitic domains. Many authors can only hope that their research retains its relevance for so long.

3.3 PHRYGIAN PRECEDENCE?

Most studies on the source of the Greek alphabet presuppose a direct adoption from the Semitic. Yet this is not at all obvious. Documented for certain in the second half of the eighth century, the earliest Phrygian inscriptions may belong in the first half if not somewhat earlier, as suggested by new results of the Gordion excavations.[237] Foreseen by Brixhe and Lejeune (1984: IX), the raising of the dates was later regarded as assured by Brixhe (2002: 24–28; I owe the

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[235] E.g., Amadasi Guzzo 1991: 304; Lemaire 2001b: 205; less resolutely also McCarter 1975: 126, and *passim*, with citations of like-minded predecessors on pp. 24–26.

[236] Excavations at Naxos yielded a MG sherd with an archaic seven-letter graffito (Lambrinoudakis 1981: 294 and Pl. 201; Johnston 1990: 466 A, reading on p. 419). The graffito was evidently incised after firing and, judging from the layout, also after the pot was broken (on this I follow Johnston, *loc. cit.*). Besides, the sherd looks very worn in the photograph. I would hesitate to found an 'aging' of the Greek alphabet on this single Naxian sherd unless it comes from an absolutely clear and absolutely clean MG context. Otherwise it cannot be ruled out that the graffito was added later, when someone picked up the sherd. See Addenda on p. 184.

[237] As is to be expected, the following relies heavily on the work of Claude Brixhe.

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reference to André Lemaire).[238] It might indeed become a certainty if the recent upward revision (Voigt and Henrickson 2000; Manning *et al.* 2001; DeVries *et al.* 2003) of the dates of the pertinent Gordion strata holds; Muscarella (2003), on the other hand, reasoned in favour of the former, lower chronology.[239] At least three inscriptions—G-237, G-239, G-104—come according to Brixhe (2002: 22) from the very first phase of level 6a, perhaps a fourth inscription as well—G-202 (*ibid.*: note 15). The letters of the earliest Phrygian inscriptions bear great resemblance to the earliest Greek letters—and obviously to the Semitic prototype. Doubts as to Greek seniority were indeed being voiced constantly, not only by authors working in Anatolia,[240] but occasionally also by classicists.[241] Yet rarely, if at all, did these doubts occupy centre stage in studies dedicated to the origin of the Greek alphabet.

The overlaps of the two scripts, chiefly the five shared vowel letters, firmly rule out an adoption from the Semitic alphabet by the Phrygians and Greeks independent of each other; likewise the deriving from Semitic *waw* of a consonant, Greek *digamma*, in addition to a vowel, Greek *upsilon* (the names of the Phrygian letters are unknown): Either the Phrygian script was adopted from the Phoenician and subsequently the Greek from the Phrygian, or vice versa.[242] But how is one to decide? As long as the date of the adoption of each script is not settled with absolute exactitude by archaeology,[243] it is the differences between the scripts that could provide an answer. These differences seem to point in the

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238 A beginning of the Phrygian and Greek alphabets earlier than the late ninth century (Brixhe 2002: 28: "tenth century?") is ruled out in any case by the Semitic prototype.

239 Some of Muscarella's archaeological and historical arguments, such as those concerning the dating of the Gordion stone reliefs or the reliability of the data in Eusebius and Africanus, seem to me equivocal.

240 E.g., Young 1963: 362–364; Barnett 1975: 433–434; Brixhe 1991a: 352–353; Lemaire 1991: 145 and note 43; Mellink 1998: 496–497.

241 Carpenter 1938: 63; Jeffery 1961: 374; 1982: 832–833; Johnston 2003: 268–269.

242 On the origin of multidirectional writing see an overlooked alternative suggested by Barnett (1975: 434): "[B]oustrophedon…in early Greek scripts, is apparently derived through [eighth-century] Phrygia from the Hittite hieroglyphs."

243 "Which script takes precedence chronologically is uncertain. Both depend mainly on stratigraphic evidence for the dates of their earliest examples…. As regards date, neither side can claim a definite seniority yet for its earliest existing inscriptions" (Jeffery 1982: 832, 833). At the time of writing the situation is perhaps a little clearer than in Jeffery's day; it might soon become evident whether the revised Gordion picture remains valid.

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direction of Phrygian seniority, though as far as I understand, the argument is not definitive.[244]

Because of this uncertainty, the possibility of an adoption of the alphabet by the Greeks and Phrygians simultaneously in the same Cilician or Tyanide milieu has occasionally been raised (e.g., Brixhe 1993: 112). It may become a serious option if Greek inscriptions of an assured early-eighth-century date are found, or conversely if the upward revision of the Gordion dates is retracted.[245]

But at present the potential of Phrygian precedence remains at least as strong as the Greek. Phrygia in the eighth century was a leading power in Anatolia, and in several respects of no lesser importance than any contemporary Greek polity. Besides, the Phoenician script was on the Phrygians' doorstep so to speak: This script coexisted with Luwian in monumental bilingual inscriptions from the late eighth century in neighbouring Cilicia and Tyana (Lemaire 1991: 133–136; 2000b: 992), and is considered to have been in use there as early as the ninth century (Lemaire

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[244] See Brixhe 1991a: 350–354 and 1994: 88, on the two letters, one for /y/ the other for /i/, which Phrygian derived from the Semitic *yod*. As Greek lacks /y/, the reasoning goes, this Phrygian particularity cannot have a Greek origin but must stem directly from the Semitic, which in turn may indicate for the Greek alphabet (where both graphemes served initially as *iota*) a dependence on the Phrygian. On the other hand the Phrygian inscriptions lack the counterparts of *zayin*, *ḥet*, *ṭet*, *samek*, *ṣade* and *qop* (Brixhe 1993: 324) present in the Greek alphabetic sequence in their 'correct' Semitic positions. This difference could indicate for the Phrygian script a dependence on the Greek, or at any rate exclude the just-mentioned opposite: "More than one trait, in effect, betrays...direct contacts between Greeks and Phoenicians, cf. the *theta*, descended from the Semitic *ṭet*, which all Greek alphabets possess and which the Phrygians did not need" (Brixhe 1995: 111). Nevertheless, the likelihood that the Phrygian alphabet in the eighth century contained all twenty-two Semitic letters in their original order is strengthened by the analogy from the amply documented Greek and especially Etruscan abecedaries. It was Lejeune (1957: 160–162; 1983; see also Prosdocimi 1990) who analyzed the 'living' and 'dead' letters in the abecedaries, their sequence and their interplay in the transition from the Phoenician to the Greek alphabet, thence to Etruscan and the other Italian alphabets, demonstrating the conservatism of the abecedaries compared with the written language at large. If the same is eventually corroborated for the Phrygian alphabet by the finding of an abecedary it will remove the above argument for Greek seniority. My warm thanks to Françoise and Dominique Briquel for instructive discussions and bibliographical references.

[245] An even more remote possibility (categorically opposed by Brixhe, *loc cit.*) would be the adoption of the Semitic alphabet and invention of the five vowel letters by a prior Indo-European people, who in turn transmitted the finished product to both Greeks and Phrygians (cf. Young 1969: 265; Jeffery 1982: 832, 833). This interesting idea should be borne in mind until eventual discoveries lend it support; for the time being it remains wholly hypothetical. Luwian Tyana, with its Phoenician inscriptions, could have been an excellent candidate. But alphabetic writing in the region until at least the late seventh century remained largely Phoenician (Cebelireis Dağı, Lawson Younger 2002) and this constitutes a strong point against a purported 'alphabetic Luwian': Had the latter indeed been devised in the late ninth or early eighth century, it would have quickly ousted other scripts in Tyana and Cilicia.

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1991: 136–138; 2001a; foreseen in Winter 1979: note 95a). And a monumental Phrygian inscription habitually dated to the late eighth century—the 'black stone of Tyana' and its more recently discovered complement—comes from the same land (on the dating see Brixhe 1991b: 44–45; Mellink 1998: 496).[246]

If it had been the Phrygians who first adapted the Semitic alphabet to an Indo-European language and had had the profound idea of the vowel letters, this people never received its due credit. While the Greek alphabet and its offshoots were to become the instrument of Western civilization, the short-lived, ill-documented Phrygian alphabet, its primogeniture ignored by the Greek tradition,[247] remained an ephemeral episode in the history of writing, known today only to a handful of specialists.[248]

3.4 THE ANTIQUITY OF THE GREEK AND PHRYGIAN ALPHABETS—SYNOPSIS

What are the results of the above, and what points must await future discoveries? I begin with the latter:

The Greek alphabet: the mother script and locale of the transmission

For the mother script there are three alternatives—exclusively Phoenician, Phoenician–Aramaic or Phrygian deriving from Phoenician.

And regarding the locale, it must have been not simply a place where educated foreign traders or mercenaries mixed for a while with their indigenous literate peers. It had to be a place where some of these foreigners stayed long enough to acquire the local language, including the ability to read and write, and to realize the potential a writing system had for themselves and their people. Only against such a background could the adoption of the alphabet to their own language, Phrygian or Greek, have taken place.

The fact that at least four different locations for the adoption could be defended so eruditely and with such excellent arguments in the scholarly literature indicates that the evidence presented thus far is perhaps less forthcoming than one would wish. That the Greek adoption of the alphabet formed part of the Orientalizing revolution does not in itself help in the quest for the mother script: Orientalizing Greece

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[246] See Brixhe 1996: 212 on the possibility of a land route of the alphabet from Cilicia via Phrygia to the Greeks.

[247] True, "*Phrygia grammata* have no sound pedigree in the Greek tradition,...and...it is worth recalling that the many and colourful Greek traditions of Midas' achievements say nothing about an alphabet" (Jeffery 1982: 833). But in no way is the issue to be decided by the words of Greek tradition alone.

[248] On the remote likelihood that some of the Hama graffiti are Phrygian see Otzen 1990: 304. Also one or two of the inscriptions on the City of David sherds which I regarded as Greek (Sass 1990) may alternatively be Phrygian. I did not mention in this chapter Lydian, Carian and other Anatolian alphabets, for now documented no earlier than the seventh century.

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evidently drew on Aramaean as well as Phoenician (and Anatolian) achievements. At present I can only underscore the necessity for future studies to confront all potential locales with each other, rather than to demonstrate the particular merits of one at a time as has been the case for the most part until now.

Phrygian or Greek seniority?

Though the present evidence does not proclaim Phrygian seniority with absolute confidence, this option might come to the forefront with more data from the Gordion excavations.

And as to the questions which seem to me settled by the available evidence—the Greek alphabet may be considered to have been adopted:

- once
- *ca.* 750 BCE or slightly earlier
- from a monumental Semitic model
- as part of the 'Orientalizing revolution'.

The one-time adoption and Orientalizing background become self-evident if an eighth-century transmission (below) is accepted. That the West Semitic model (adopted by the Greeks either directly or via Phrygian) was monumental has been established by Naveh, following Carpenter. And as to the time of the transmission—for straightforward Hellenic as well as Semitic considerations, all known in 1973, a dating of the Semitic prototype *ca.* 1100 BCE was never an option at all. The Proto-Canaanite-like appearance of some eighth–sixth-century Greek letters is caused either by long-lived Semitic traits, or by still later local Greek developments. Moreover, whereas seventeen of the twenty-two adopted Greek letters are chronologically unhelpful, West Semitic comparisons date the remaining five, taken together, to the first half of the eighth century or at most *ca.* 825–750. Yet on the Hellenic side the lack of Middle Geometric pottery with Greek inscriptions seems to preclude a transmission much earlier than the mid eighth century (but see p. 154).

And so, equating the birth-date of the Greek alphabet with the time of the earliest Greek inscriptions *ca.* 750 (or slightly earlier) not only suits the Hellenic circumstances; it is in full agreement with the Semitic side. But should future developments in the Greek field require us to raise the age of the Cadmean letters by several decades, Semitic epigraphy will not stand in the way.[249]

The just-mentioned Phoenician–Aramaic equivalents afford welcome flexibility to the birth-date of the Phrygian alphabet as well: In case this alphabet is eventually

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[249] The last sentence is an adaptation from my earlier conclusions (1991a: 98).

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found to have preceded the Greek, the said Semitic comparisons go back a maximum of *ca.* 75 years—to around 825 BCE.

All this also accords well with the contemporary West Semitic cultural and political situation, firmly in place since the ninth century—but not earlier: territorial states with their royal cities, monuments and administrations and the ensuing proliferation of writing, and the Phoenician westward expansion (see pp. 64–68). The transmission, moreover, is appropriately set in the time of intensive Greco–Levantine interaction during the early Orientalizing period in Greece. These reasons for a date *ca.* 750 BCE are not all too far from those by which Carpenter in 1933 and 1938 backed his proposed second half of the eighth century.

As West Semitic alphabetic writing in the ninth century was well established in the Levant, and to some extent along the Mediterranean shores, it could hardly have escaped the attention of the well-travelled Greeks. One might ask, then, why it took these enterprising people another 50 or 100 years to avail themselves of so advantageous an instrument? Whereas at the moment I can offer no answer, I would point out one presumption that is categorically to be ruled out. It is impossible to settle the question by assuming an inscriptional void during a purported formative period of the Greek alphabet, say the maximum range of *ca.* 825–750 BCE allowed by the Semitic comparisons. I take it that the Greek (and Arabian) alphabets spread practically immediately. Once the need for literacy was felt and fulfilled, there was no reason why the Greeks should lag behind the inhabitants of the eastern Mediterranean in things alphabetical. If this is accepted, the oldest extant inscriptions should date approximately to the time of the transmission. And as long as no inscribed Middle Geometric pottery is known, the Greek alphabet is in any case unlikely to be much earlier than the mid-eighth century.

These issues can be developed further only within the study of Geometric chronology (e.g., a revision of the MG–LG transition upward from 750 BCE—see p. 154), and of the causes and beginning of the 'Orientalizing revolution'. In other words, a stability of the present Geometric chronology may constitute a point in favour of Phrygian seniority.

Finally, I return to the extreme attempt by classicists to counter Naveh's idea of an 1100 BCE transmission of the alphabet to the Greeks. This is the verdict, arising no doubt from desperation, regarding there being too few Semitic inscriptions to draw conclusions about the date of the transmission (e.g., Burkert 1992: 28; Powell 1997: note 30; Morris 2000: 262–263; Whitley 2001: 130). From where did Morris, to name one, take his claim that "we know of fewer than a dozen west Semitic inscriptions dating before 500 BC"? This verdict is of course groundless; not so, however, the sentiment that gave rise to it. No wonder classicists were baffled by Naveh's 1973 thesis, which went against the grain of the Greek evidence—epigraphy, culture and

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history—then by the seeming shake-up of Semitic epigraphy following the 1979 discovery of the Fekheryeh text. The impression that this field was erratic arose inevitably (see pp. 71–72 on this false impression and its genuine underlying troubles). I cannot end this appraisal of Greco–Levantine epigraphic links and their chronology without expressing my hope that it renders the classicists' bewilderment in face of Semitic epigraphy a thing of the past.

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EXCURSUS F. ON PROTOGEOMETRIC AND GEOMETRIC CHRONOLOGY

Greek chronology from Late Helladic IIIC to Late Geometric is still founded to a great extent on the long-known absolute dating of the beginning and the end of this continuum, combined with an estimate of the mostly 'floating' duration of each phase and sub-phase (see Lemos 2002: 24–26). This excursus is concerned chiefly with the end of Protogeometric (for the Tekke bowl) and the beginning of Late Geometric (for the earliest Greek alphabet).

Traditionally the end of Attic PG is set at 900 BCE, with the next phases as follows:

Early and Middle Geometric	900–750
Late Geometric	750–700

Greco–Levantine synchronisms

The absolute dating of the Protogeometric-to-Geometric transition to 900 BCE was arrived at via Greek pottery excavated in the Levant in the 1920s and 1930s in what have since turned out to be inadequate archaeological contexts. Moreover, the Greek dating was based on a Palestinian chronology held by Kenyon for some time, but when Kenyon subsequently changed her mind, joining most Palestinian archaeologists in raising the absolute dates by a full century (Yadin's 'Hazor, Megiddo and Gezer' idea), this was ignored by the classicists. The inevitable conclusion of Lemos (2002: 25–26) was that:

> [F]or PG…no absolute dates can be established by cross-dating with finds from sites in the eastern Mediterranean. PG pottery has been found in Cyprus, Syria, and Palestine, but its context is either not related to well-dated historical events or is problematic for some other reason.

The corrected chronology of Palestinian archaeology (Finkelstein 1996) has once again brought the Levantine and Greek time-frames closer together—cf. Fantalkin 2001. Lemos (2002: notes 160, 161) was aware of this chronology, and of Fantalkin's paper while it was still in press, but their implications did not yet find full expression in her book. Recent investigations of the Greco–Levantine synchronisms may be found in Fantalkin 2001: 121, and *passim*, Gilboa and Sharon 2001 and 2003, and Coldstream and Mazar 2003 (on the chronology cf. also Finkelstein and Piasetzky 2003). Fantalkin's study supersedes in part the chronological discussion in Kopcke 2002, that was only moderately updated beyond 1995 (cf. Kopcke 2002: note 1).[250]

Greek chronology and the Italian Iron Age (see also Excursus G)

The new ^{14}C chronology of the Italian early Iron Age moved this period away from Late Geometric, the phase when Greco–Italian material-culture synchronisms, as

[250] Alexander Fantalkin advised me on Greek chronology, for which I am most grateful.

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documented in excavations in Italy, begin in earnest (Nijboer *et al.* 2000: 174). A certain upward shift in the initial date of LG may thus suit the Greco–Italian link.

Points for future study

Palestinian archaeology and West Semitic epigraphy can live with the current dates for LPG and LG. All the same, a Protogeometric-to-Geometric transition slightly later than 900 BCE seems preferable from a Palestinian viewpoint if the recent ^{14}C chronology (see p. 66 and note 108) holds. This lowering could also suit the Tekke bowl and its inscription (pp. 34–36). On a similar note, moving the beginning of Late Geometric, hence of the genesis of the Greek alphabet, a few decades up from 750 BCE might fit better with the West Semitic roots of that alphabet.

Time will tell if these changes, favoured on the Semitic side (as I see them), are indeed supported, or at any rate tolerated, by an eventual updated Greek ^{14}C chronology. For now I hope that even under the present system, classicists will be able to accommodate in just one century the EG and MG developments combined (allotted 150 years in the prevalent chronology), with the remaining 50 years divided between LPG and LG, as follows:

End of Protogeometric	880/870 (now 900)
Beginning of Late Geometric	780/770 (now 750)

That the duration of PG is thus lengthened downwards and the duration of LG extended upwards, each by two to three decades, need not have repercussions on the customary beginning of PG *ca.* 1000 BCE or the end of LG *ca.* 700, nor on eventual ^{14}C-induced changes therein. After all, the dates of the beginning and the end of the Late-Helladic-IIIC-to-Late-Geometric continuum remain as they were. It is only the internal absolute dates that may shift, with a certain crowding of the sub-phases towards the end, when the pace of change in the pottery styles probably accelerated.

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EXCURSUS G. THE OSTERIA DELL'OSA GRAFFITO

The graffito and its context

The Osteria dell'Osa cemetery in Latium was excavated between 1971 and 1986 (Bietti Sestieri 1992: 79). A jug of local manufacture from Tomb 482 carries a graffito incised after firing (*ibid.*: 184). The first sign resembles a *he* or *epsilon*, the last—a *nun* or *nu*; the interjacent signs, two or three—the number is uncertain—are too indistinct (Fig. 29). The tomb belongs to the end of Latial II, i.e., IIB2, a phase that according to the so-called traditional chronology of Italy lasted *ca.* 800–770 B.C.E. (variant 825–800) (cf. Carandini 1997: xxx).

The script

Several specialists took the script of the graffito to be Greek, and if so it is older than any known alphabetic text in that language (e.g., Amadasi Guzzo 1991: note 18; 2000: 235; Ridgway 1996; the latter reference I owe to Alexander Fantalkin). I am rather skeptical: The Greek attribution cannot be ascertained, and the one or two attempted decipherments as Greek (cf. *Supplementum Epigraphicum Graecum* 48 [1998], no. 1266) are little more than shots in the dark. Against this background the contention that the text employs "at least one vowel" (Johnston 2003: 263) may be premature.

Absolute chronology

As no Greek inscriptions on Middle Geometric pottery are known (see note 236), it would be rash to establish the upward revision of the adoption of the Greek alphabet beyond 750 BCE on this (and perhaps another) brief, nondescript scribble on a non-Greek object from a non-Greek context. For the time being it may be preferable to regard the Osteria dell'Osa graffito as a pseudo-inscription—an ineffectual attempt by an illiterate person to reproduce some letters. As to the alphabet imitated there are two possibilities:

1. In case the vessel is later than Tomb 482 (see for instance Bartoloni and Nizzo 2003: note 20), a Greek (or Etruscan?) model for the graffito is not impossible.
2. But if the attribution of the vessel to the tomb remains valid, the graffito is likelier to be pre-Greek, i.e., a retrograde imitation of Phoenician letters. That

Fig. 29. The Osteria dell'Osa graffito.

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the inhabitants of Latium in the ninth century were exposed to the Phoenician alphabet is possibly indicated by the presence, across the Tyrrhenian Sea, of the Nora and Bosa inscriptions (see p. 68).

What is more, the traditional chronology of the Italian early Iron Age has been challenged in the last decade by recently calibrated ^{14}C dates. Carandini (1997: xxx), and Bettelli (1997: 598), cited studies which set Latial IIB2 in the ninth century (either 910–880 or 850–810 cal BCE). Nijboer *et al.* (2000: 170, and *passim*) concluded that Latial II ended *ca.* 820 BCE at the latest. Similarly Attema *et al.* 2002: 323–325. These dates—if they prevail, and in case the graffito indeed belongs to the said Latial phase—firmly exclude any connection between the Osteria dell'Osa graffito and the Greek alphabet; they buttress instead the (admittedly less exciting) pseudo-Phoenician option.[251]

[251] I am obliged to Dominique Briquel, who initiated me into the *status quaestionis* of early Iron Age Italian chronology.

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AFTERWORD

The 'traditional' chronology of the West Semitic alphabet *ca.* 1150–850, hinging on the 1000 BCE dating of Ahiram, was perfected gradually over many decades. It was to some extent justifiable that when the first contradicting evidence appeared, attempts were made to somehow fit it into the prevailing scheme. But then the contradictions multiplied: For the past quarter century the 'traditional' chronology has been waging an uphill battle against a changing concept of the ancient Levantine social and political picture, and against a growing number of archaeological and palaeographical finds and phenomena, both new and long-known, that were persistently 100–200 years too young. In its attempts to dismiss the former and 'age' the latter, this high chronology has become increasingly unworkable.

The framework presented herein seeks to rid the perception of the alphabet's absolute chronology *ca.* 1150–850 BCE of the above phantom discrepancies and to re-synchronize the palaeographical developments with the archaeological and historical evidence. Invoking the archaeological context of the Tekke and Kefar Veradim bowls and the art-historical dating evidence for Ahiram, this framework ascribes to the first half of the ninth century the evolution of the West Semitic alphabet from Proto-Canaanite to the Aramaic–Phoenician and Hebrew scripts, and links this phenomenon to the contemporary emergence of Levantine territorial states and their administrations. Ultimately, the Arabian, Phrygian and Greek alphabets too are derivatives of these developments.

Comparisons with the shapes of several West Semitic letters led no few Semitists to favour a late-second-millennium adoption of the alphabet in Arabia; others (though fewer) advocated a similar dating of the Greek alphabet. That the West Semitic alphabet was the ancestor of the two said scripts is agreed by all; what I found untenable were the high date assigned the West Semitic prototype and the unbefitting historical background thus imposed on the adoption of either alphabet.

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Some of the earliest South Arabian letters recall West Semitic shapes straddling the transition from Proto-Canaanite to Phoenician–Aramaic, or the first half of the ninth century. Combined with the Arabian archaeological evidence, the lower end of that range, *ca.* 850 BCE, appears to me the most likely time for the adoption of the alphabet in Arabia. I further hold that the monumental script is either contemporary with or only slightly later than the adoption itself.[252] And it is understood that the seniority of the South Arabian alphabet vis-à-vis the less-well-known North Arabian can no longer be argued *a priori*. I believe that the birth of both alphabets was contemporaneous.

Semitic epigraphical comparisons allow the genesis of the Greek alphabet a range of *ca.* 825/800–750 BCE. A closer dating is only possible via the Greek texts themselves, of which the earliest currently coincide with the lower end of the said range. The transmission of the alphabet to the Greeks evidently formed part of the Orientalizing phenomenon. The Greek alphabetical traits bearing a formal resemblance to Proto-Canaanite shapes, hence thought by Naveh to date *ca.* 1100 BCE, are in fact not Semitic in the least: Classicists always deemed features such as boustrophedon writing and the legless *mu* post-transmission inner Greek developments, and, I believe, with good reason.

Lastly, the oft-overlooked Phrygian alphabet: As the oldest inscriptions in that script also belong to the mid-eighth century at the latest, the question of who first adapted the Semitic alphabet to an Indo-European language, devising the five vowel letters, remains open for now. But the potential of Phrygian precedence is at least as strong as the Greek.

It is my hope that the chronology proposed in the preceding chapters provides an inclusive working hypothesis, with all its components operating in unison, for the alphabets in the Levant, Arabia and the Greek world at the turn of the second millennium and the beginning of the first.

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[252] The letters of the earliest monumental inscriptions do not look less archaic than those of the earliest South Arabian inscriptions at large. But as the Arabian archaeological picture is still full of gaps, these conclusions have yet to be corroborated by the results of further excavations and research.

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LIST OF TABLES AND FIGURES

TABLES

1. Archaizing, non-archaizing and 'eccentric' letters in the Byblos inscriptions 23
2. Diagnostic Byblos letters and comparisons (B. Sass) 24
3. Archaizing, non-archaizing and 'eccentric' letters in the Tell Fekheryeh inscription 41
4. Key datings of West Semitic inscriptions 73
5. Five inscribed sherds from Hazor 86
6. Four alternative Gozan chronologies 95
7. The time-lapse between the earliest South Arabian states and Karib'il Watar 117
8. Diagnostic early Arabian letters and their West Semitic comparisons (B. Sass) 121
9. Early links between the South and North Arabian scripts (B. Sass) 126
10. The twenty-two Greek letters from *alpha* to *tau*—nature of their Semitic derivation 141
11. Diagnostic early Greek letters and their West Semitic comparisons (B. Sass) 144
12. The post-transmission modification and ensuing Proto-Canaanite look of three Greek letters (B. Sass) 145

FIGURES

1. Schematic family tree of the early alphabetic scripts (B. Sass) 12
2. Byblos, Abibaal (Clermont-Ganneau 1903: 381, 382) 18
3. Byblos, Elibaal (Montet 1929: pl. 36) 19
4. Byblos, Ahiram banquet scene (Muyldermans 1989: 407) 20
5. Nimrud, banquet scene on pyxis from Well AJ (*loc. cit.*) 21
6. Byblos, Ahiram sarcophagus inscription (Vincent 1925: pl. 8) 26

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7. Byblos, Ahiram graffito (Montet 1929: pl. 126) 28
8. Byblos, Yahimilk (*Byblos* I: pl. 31) 28
9. Byblos, Šipiṭbaal (Dunand 1945: pl. 16) 29
10. Byblos, Bordreuil champlevé inscription (Bordreuil 1977: pl. 5) 30
11. Tekke bowl (Sznycer 1979: pls. 1, 2) 35
12. Kefar Veradim bowl (Alexandre 2002b: figs. 3, 4) 37
13. Fekheryeh inscription (Abou Assaf, Bordreuil and Millard 1982: unnumbered page towards end of book) 42
14. Byblos cone A (*Byblos* II: pl. 144) 44
15. Byblos, Ahiram sarcophagus (Montet 1929: pl. 128) 79
16. Taanach, Lapp's cult stand (Hestrin 1987: 63) 80
17. Ebla basin (Matthiae 1965: pl. 69) 81
18. Ebla statue base (Matthiae 1998: 570, 571) 82
19. Five inscribed sherds from Hazor (*Hazor* II: pl. 169: 1–4; *Hazor* III–IV: pl. 357: 1) 85
20. Yalâ, inscribed sherds from Level B (Garbini 1992: 80) 97
21. Yalâ, inscribed sherds from Level A (Garbini 1992: 81–84 [selection]) 98
22. Yalâ, decorated and inscribed terracotta plaque from Level A (Antonini 1996: figs. 1, 2) 100
23. Raybûn, sherds with painted inscriptions (photographs—Sedov 1996: figs. 5: 1, 6 top left; drawings—Sedov 1997: fig. 40) 102
24. Hajar Ṣurbân, cooking-pot with incised inscription (Breton and Darles 1994: fig. 5) 105
25. Al-Midaman, inscription on openwork vessel or lid (Keall 1998: fig. 10d) 106
26. Es-Sawdâ', monumental inscriptions (photograph—Breton 1996: 99; drawing—Breton 1990: seventh unnumbered page) 108
27. 'Baal Lebanon' bowl inscription (*CIS* I 5) 135
28. Athens, Dipylon jug inscription (Jeffery 1982: 829) 135
29. The Osteria dell'Osa graffito (Bietti Sestieri 1992: 185) 155

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AAE. Arabian Archaeology and Epigraphy.

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AJA. American Journal of Archaeology.

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AOAT. Alter Orient und Altes Testament.

AoF. Altorientalische Forschungen.

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BaM. Baghdader Mitteilungen.

BAR. Biblical Archaeology Review.

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DaM. Damaszener Mitteilungen.

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IEJ. Israel Exploration Journal.

IstMitt. Istanbuler Mitteilungen.

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OBO. Orbis Biblicus et Orientalis.

OLA. Orientalia Lovaniensia Analecta.

OLZ. Orientalische Literaturzeitung.

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ADDENDA

To p. 38. That gadrooned bowls were already known in the Late Bronze Levant (in addition to contemporary Egypt) is illustrated by a specimen from Ugarit. Discovered in 1963 and recently published (Galliano 2004), this miniature bowl is made not of metal but of stone or glass (the material has not been securely identified). This has no bearing on the dating of the Kefar Veradim bowl, however, whose parallels are clearly Iron II, as noted. My thanks to Geneviève Galliano and Velérie Matoïan for discussing the Ugarit bowl with me.

To p. 51, note 65. New data and a new look at old data undertaken in 2004 possibly demonstrate now (Sass forthcoming) that the previously proposed Middle and Late Bronze palaeographic stagnation was a phantom.

To p. 92. Reliefs strongly linked with Ain Dara and an associated Luwian inscription of Taitas, king of the land Wadasatini, were uncovered in Aleppo in 2003 (Kohlmeyer 2004; David Hawkins in a lecture at the Oriental Institute, Chicago, 5 March 2005). This king and his land are already documented at the Amuq and Hama (Hawkins 2000: 365, 403). Hawkins now proposed an eleventh-century dating for Taitas (pers. comm. quoted in Kohlmeyer 2004; Chicago lecture). If this dating is corroborated by other than stylistic considerations, my much lower setting of Ain Dara and the related conclusions in Excursus D will have to be revised or discarded.

To p. 106, Section 2.1.5. For further early monumental inscriptions, discovered in 2004 in the *intra muros* temple at Sawdâ', see Audouin and Arbach 2004. The reliefs which these inscriptions accompany in several aspects revolutionize our knowledge of South Arabian imagery; thus for instance the compositions of two seated, facing deities—the latter identified by inscriptions—are clearly borrowed from early-first-millennium Syro–Hittite banquet scenes, even though in the latter it is humans who are depicted.

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To p. 131. Comments by Françoise Briquel-Chatonnet, François Bron and Christian Robin in January 2005 made it evident to me that the following requires more emphasis: The ductus of the Raybûn pottery inscriptions, themselves amongst the earliest Arabian inscriptions, is nearly identical to the monumental 'pre-classical' ductus. It is thus impossible to maintain the Assyrian synchronism while at the same time dating the Raybûn pottery inscriptions much earlier than 800 BCE. A potential middle ground, stretching both the synchronism and the West Semitic parallels to the limit but perhaps just barely accommodating both, would be to date the genesis of the Arabian alphabet towards 800 BCE and the earliest monumental inscriptions very shortly thereafter. Support for such a notion may eventually emerge from the growing documentation of several generations of pre Karibʾil Watar rulers in Saba and of their Nashshan contemporaries (e.g. Robin and Arbach forthcoming). Yet the present evidence still seems to me to prefer a dating *ca.* 850 BCE for the genesis of the Arabian alphabet, and before 800 for both Raybûn and the earliest monumental inscriptions (see pp. 114, 117–118).

To p. 134. Lipiński (2004, 47–48) was for a slight raising, within the first half of the eighth century, of the date of the Baal Lebanon inscriptions. This may indeed suit the palaeography better.

To p. 146, note 236. The finding of twenty-six early Greek inscriptions in the sanctuary of Apollo Daphnephoros, Eretria, was announced in Theurillat 2004 (reference kindly sent by Alexander Fantalkin). Twenty-five among the inscriptions were attributed to Late Geometric and a single one, "clumsily written...on an ostrakon," to Middle Geometric II. All (or most) of them were said to come from pits. Raising the date of the earliest Greek alphabet from LG to MGII on the strength of a single, clumsily written text on a sherd from a pit is not impossible (it is still in line with the dating of the Semitic prototype), but it has to be carefully substantiated. A convincing description of the stratigraphy, a photograph of the inscription, and a convincing identification of the latter as Greek must be awaited before the current notion of a Late Geometric genesis of the Greek alphabet can be safely abandoned in favour of a Middle Geometric II beginning.

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INDICES

General Index

^{14}C see radiometry
22nd dynasty (Egyptian) 15[9], 22, 66[103]
Abdô sherd 15[10], 25, 30–32, 46, 84[+133], 85
Abecedary 110[172], 122[+196], 124, 148[244]
Abibaal 15, **16–17**, 18, 20[+12], 23[15], 24, 32[28], 46[50], 47[+52, 53], 49[56], 70
Abisalamu 93, 95
Acre 36
Adad-nirari II 56, 63, 93, 94[152], 95
Adad-nirari III 94, 95
Afis, Tell 64
Africanus 147[239]
Ahiram 11, 13[+4], 14[+7], 15, **17–22**, 23[15], 24, 26, 30, 31[+26], 32[+28], 33, 36[33], 38[+39], 39, 45, 46[50], 47[+53], 48, 49, 58, 61, 68–73, **75–82**, 90[+142], 119[189], 135, 157
– Ahiram graffito 15[10], 28, 30, 47[54]
Ahlamu 62, 90
Ahuni 57
Ain Dara 11, **89–92**, 183
Alabaster 77, 114[178]
Alaca Höyük 91
alâni dannûti 65
alâni šarrûti 65
Albright, William Foxwell 131
Aleppo 89[138], 90[+140], 91[+146], 183
Alexandre, Yardenna 36[34]
Alphabetic cuneiform (see also Ugaritic) **53–54**, 59[85], 70, 122[196]
Alphabet, long 122[+196], 123
Alphabet, short 54, 59[85], 122[196], 135
Amal, Tel 87
Amman Citadel inscription 83
Amorite 63[95]
Amuq (see also Patin) 139, 183
Amurru 89
Anatolia(n) 57[79], 90[139], 139, 147, 148, 149[248], 150
Apollo Daphnephoros 184
Arabia(n) / South Arabian (see also North Arabian, Saudi Arabia, Yemen) 9, 11, 12, 13[3], 65, 69, 71–73, **96–132**, 151, 157, 158, 183–184
Arabian trade (diverse terms) 112, 115, 116, 118, 127, 130
Arad 66[103], 87, 88
Aram, Aramaean, Aramaic (see also Ahlamu, Phoenician–Aramaic) 12, 13–74 (*passim*), 85, 86, 87, 89[+138], 90, 92, 93, 94[152], 112[174], 123, 129, 138, 139[+224], 145, 150
Arbil 62
Archaeological Institute of America 143[230]
Aroer 112, 114[178]
Arpad 89[138]
Arrowhead 14, 15, 24, 34, 39[43], **43–44**, 45, 52, 53, 55, 72, 73, 121[+193]
Arslan Tash 83
Arslantepe see Melid
Arwad 139
Ashara see Terqa
Ashur-dan II 64
Aššur-dan-apli 95
Assur (city) 62
Assur-bel-kala 62
Assurnasirpal II 56[75], 63, 64, 65, 90
Assyria(n) 15, 22, 36, 38[+36, 40], 48, 51[63], 52[67], 54, 56[+75, 78], 57, 58, 60,

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Original from UNIVERSITY OF MICHIGAN

61[+91], 62–64, 65[100–102], 66, 67, 89–94[+152, 153], 95, 115–118, 126, 130, 184
Athtar ḏû Riṣaf 106
Attic(a) 34, 153
Awsan 116[182]
'Baal Lebanon' 134, 135, 142, 144, 184
Babylon(ia/n) 44[48], 126
Bahianu 95
Balih 62
Banquet scene 20, 21[+14], 22, 45, **75–77**, 81, 82[+128], 183
Barrakab 27[+20], 144
Batash, Tel 87
Beersheba 112, 114[178]
Beirut 66
Beth Shemesh 87, 110[172], 122[+196]
Bit Adini 94[+152], 95
Bît akîtu 115
Black on Red pottery 39[42]
Boaretto, Elisabetta 69[108]
Boğazköy 81
Bordreuil, Pierre 9
Bordreuil champlevé inscription 15[10], 23[15], 25, 30–33[+29], 45, 71
Bosa (see also Sardinia) 33[30], 83, 156
Bowl (bronze) see Kefar Veradim, Tekke
Brandl, Baruch 38[38]
Briquel, Dominique 9, 148[244], 156[251]
Briquel-Chatonnet, Françoise 9, 21[14], 148[244], 184
Brixhe, Claude 146[237]
Bron, François 9, 96, 116[182], 125[202], 184
Bulla 51[64]
Byblos (see also pseudo-hieroglyphic) 13–82 (*passim*), 83–86, 119[189], 121
Byblos cone A 15[10], 44, 52
Byblos cone B 15[10], 84
Byblos spatula 15[10], 24
Cabul, Land of 36[35]
Camel 118, 130
Canaanite 130, 135, 137[218]
Carchemish 56[75], 57, 62, 67[+105], 89[+137, 138], 90, 91, 92[+148]
Carian 149[248]
Cebelireis Dağı 148[245]
Cheniour, Nadia 9
Cilicia(n) (see also Que) 9, 139[+225, 226], 140, 148[+245], 149[246]
Cisjordan 66
City of David 149[248]
Collège de France 9
Crete 34, 137[218]
Cuneiform (i.e., Akkadian; alphabetic cuneiform under alphabetic) 40, 56[+78], 62, 63, 70
Cursive (South Arabian) 97, 111[+173], 119, 121, **124–128**, 130
Cypro-Phoenician pottery 39[42]
Cyprus 33[30], 58[82], 68, 71, 83, 153
Damascus/Damascene 57[81], 59[+85], 60, 67, 69
Dan (tel and stele) 22, 25, 30, 32, 33, 50, 52[+68], 55, 58, 65, 83, 86, 87
Dan sherd 55, 83, 84
David 66
Dedanite 128[209], 131
Dendrochronology 66
Dipylon jug 134, 135, 137[218], 142, 144
Dius 67
Dor, Tel 35, 66, 67
Dridi, Hédi 9
Dromedary see camel
ḏt Ḥmym 103[165]
Early Geometric see Geometric

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Ebla 79, 81, 82
Egypt(ian) 15[9], 48, 61[91], 77, 126, 183
Elibaal 15, **16–17**[+11], 19, 20[+12], 23[15], 24, 30, 32[28], 46[+50], 47[+53], 48, 49[+56], 70
Empire see Hittite
En Gev sherd 55, 83, 84
Epstein, Marc 9
Eretria 184
Eshtemoa 87
Ethiopia 122, 126[204]
Etruscan 148[244], 155
Euboea 35, 139[226]
Euphrates 56, 62, 89[137]
Eupolemus 67
Eusebius 147[239]
Fantalkin, Alexander 9, 153[250], 155, 184
Fauveaud, Catherine 9
Fekheryeh 14[8], 25, 26[+17], 32, 34, 39, **40–43**, 46[+51], 48–50, 52[+68], 56, 58, 59, 61–63, 70, 71, 83, **93–95**, 135, 139[228], 143, 152
Finkelstein, Israel 9
'Flame and frond' ivory group 76[+118, 119]
Forgery 33, 34, 39[43]
Gajda, Iwona 9, 96
Galilee 36, 70, 86, 87
Galliano, Geneviève 183
Geometric period (see also Protogeometric) 146[+236], 150, 151, **153–154**, 184
Gerbaal arrowhead 24
Gezer 84, 86, 87, 101, 110, 153
Gilboa, Ayelet 69[108]
Girou, Françoise 9
Glyptic see seal
Gordion 146, 147[+239, 243], 148, 150
Gozan 11, 26[+18], 40, 43[47], 56, 57, 61, 62[92], 76[+118], 84, 92[148], **93–95**, 115
Greece/Greek 9, 11, 12, 13[3], 14[+8], 35, 66, 69, 71–73, 113[176], 114, 115[181], 116[183], 119[188], 122[195], 123, 131, 132, **133–156**, 157, 158, 184
Gurgum 89[137, 138]
Guzana see Gozan
Habur 43[47], 62, 94[152]
Hadar, Tel 35
Hadianu **93–95**
Hadramawt 100, 101[161], 104, 109
Hadyisʿi 43, **93–95**,
Hajar Bin Humayd 97, 99, 101, 104, 109, 110, 113, 115[180], 128, 130
Hajar Ṣurbân **104–105**, 111
Halaf, Tell, see Gozan
Hama/Hamath 59, 89[138], 90, 149[248], 183
Hanigalbat 93
Ḥarâshif 125, 126
Hattuša 89[+137], 91
Hawkins, David 183
Hawley, Robert 9
Hazael 25, 52[68], 83, 87, 121
Hazor 32, 36, 55, 83, 84, **85–87**
'Hazor, Megiddo and Gezer' 86, 153
Hebrew 11, 12, 27[20], 32, 33, 38[40], 40, 44, 45, 51, 52, 55, 58[83], 60[+87], 68, 69, 70, 72, 74, 83, **87–88**, 123[+199], 129, 157
Heirloom 22, 39[+42], 71, 73, 77
Helladic see Late Helladic
Hellenistic 67
Hindanu 118
Hiram 36[35], 66, 67
Hittite empire 61, 62, 92[+148]
Hodayda 105

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Horvat see under second word
Hurrian 57[79], 61[91]
Ibex 110
Incense trade see Arabian trade
Indo-European 139, 148[245], 149, 158
Ini-Tešub 90
Institut d'études sémitiques 9, 96
Iran 110
Iraq 114[178]
Ischia 139[226]
Israel (northern kingdom) / Israelite (see also Samaria) 58, 70, 63, 86, 87, 88, 112[174]
Israel (present state) 34, 35, 54, 59, 112
Ita'amra see Yiṯa'amar Bayyin son of Sumuhu'alî
Italy/Italian 9, 148[244], **153–154**, 156[+251]
Ittobaal 20[12], 47
Ivory (see also 'flame and frond') [21], 22, 72, **75–76**, 77[+122]
'Izbet Ṣarṭah 15, 31, 43[45], 44[+49], 45, 69, 145
Jabal an-Nisîyîn 104
Jebel Bishri 112[174]
Jezireh 56, 63, 94
Jezreel Valley 53, 70, 86, 87
Jordan (present kingdom; see also Transjordanian) 112
Jordan River and Valley 70, 87, 88
Josephus 67
Judah(ite) 59, 70, 87, 88
Kamna 107[170], 126
Kapara 56, 76[118], **93–95**
Karatepe 75[115], 144
Karib'il Watar son of Ḏamar'alî (Assyrian Karibilu?) 115[+181], 116, 117, 184
Karkamiš see Carchemish
Katuwas 56[75]
Kazel, Tell 67[104]
Kefar Veradim 4, 15, 16, 24, **36–39**, 43, 45, 48, [49], 52, 55[74], 69, 70, [72], 73, 121, 157, 183
Kenyon, Kathleen M. 153
Khadr, el- (arrowheads) 121
Kinnerot, Tel 44, 83
Knossos 34
Kulamuwa orthostat 25, 30, 32, 33[+30], 48, 52[68], 55, 83, 121, 125, 144
Kulamuwa gold ferrule 25, 52[68], 83
Kummuh 89[137, 138]
Laboratoire des études sémitiques anciennes 9
Lachish ewer 120[+192], 121
Lamashtu 97[156]
Lapp, Paul 80
Late Geometric see Geometric
Late Helladic 153, 154
Late Hittite see Luwian
Late Protogeometric see Protogeometric
Latial/Latium 155, 156
Lebanon see Baal Lebanon and Mount Lebanon
Lemaire, André 9, 147
Letter order (*hlḥ* and *'bg*) 110[172], 122[+196, 197], 123
Levant(ine) 9, 11, 13–74 (*passim*), 75[116], 82, 91[144], 96, 107, 109, 110, 112, 113[+177], 115, 116, 117, 118[+185], 119, 122, 123, 129, 130, 139, 151–153, 157, 158, 183
– southern Levant 13–74 (*passim*), 90, 92[+150], 130
Ligature 85
Lory, Jean-Luc 9

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Louvre 23[15]
Lozachmeur, Hélène 9
Luwian (see also Anatolian, Syro-Hittite) 51[63], 53, 54, 56–67, 70, 76[120], 78, **89–92**, 94[152], 139, 147[242], 148[+245], 183
Lydian 149[248]
Macalister, R.A.S. 101, 110
Macdonald, Michael 124, 131
Maison des Sciences de l'Homme 9
Maison Suger 9
Malatya see Melid
Marib 97, 109
Mater/matres lectionis 43, 51, 52[+67], 138, 139[+224, 225]
Matoïan, Valérie 183
Mediterranean 139, 151, 153
Megiddo 36, 44, 55[72], 75[+113, 114], 86, 153
Melid 61, 67[+105], 89[+137, 138], 91, 92
Menander of Ephesus 67
Mesha stele 15, 22, 25, 50, 52, 58, 83, 88, 121
Meskene 89[137]
Mesopotamia(n) (see also Iraq) 64, 111, 112[174], 113, 115[180], 116, 118, 125[202], 127, 129
Midaman, al- **105–106**, 111
Midas 149[247]
Middle Bronze Age 77, **78–82**, 107, 183
Middle Euphrates see Euphrates
Middle Geometric see Geometric
Midianite pottery 101[161], 104, 110
Mina, al- 138[222]
Minaeo–Sabaean 131
'Minuscule' see cursive
Moab(ite) 88
Mokha 105
Mount Lebanon 62
Mukiš see Patin
Nabonidus 127
Nashshan 184
Naxos 146[236]
Neo-Hittite see Luwian
Neolithic 103[163]
'Nestor's cup' 137[218]
'New Canaan' 44, 59, 66, 67, 92[150], 119
New Kingdom 77
Nimrud ivory pyxis 21, 22, 49, 72, **75–76**, 77[122], 82
Nineveh 62
Nora (see also Sardinia) 14[8], 33[30], 58, 68[+106], 71, 72, 83[+131], 156
North Arabian, northern Arabia 100, 104, 112[+174], 113, 118, **124–128**, 130, 131, 158
Obsidian 101, 103[163]
Omride 66, 70, 86, 87
Ördek Burnu 84
Orientalizing 137, 149, 150, 151, 158
Orman, Tallay 38[38]
Osorkon I 15[+9], 16, 17, 22, [47], 49[56], 70, 73
Osteria dell'Osa 9, 11, 140, 146, **155–156**
Palestine 11, 13–74 (*passim*), 83[130], 96, 101[+161], 104, 110, 112[+174], 119, 130, 131, 153, 154
Palm-stalk 97, 121, **124–128**, 130, 131
Panamuwa 144
Papyrus 50, 51[+64], 53, 54, 58, 60, 124
Paris 9, 23[15], 96
Patin(a) (see also Amuq) 89[137, 138], 91[+143]
Penchas, Rodica 4, 9
Persian period 27[20], 38[36], 51[64], 139

Digitized by Google
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Philistine 83[130]
Phoenicia(n) 12, 13–74 (*passim*), 75[114], 78, 82, 83[130], 85, 86, 119[187], 120[192], 123[+199, 200], 127[206], 129, 134, 136–140, 145, 148[244, 245], 149, 150, 155, 156
Phoenician–Aramaic 11[+2], 12, 13–74 (*passim*), 138, 139, 140, 142, 145, 149, 150, 157
Phoenician westward expansion 14[+8], **68**, 71, 139, 151
Phrygia(n) 11, 133, 138, 139[+228], 140, 144, 145, **146–149**, 150, 151, 157, 158
Pirenne, Jacqueline 115[181], 116[184], **131–132**
Pollak, Myrna 9
Proto-Canaanite (see also Canaanite) 11, 12, 13–74 (*passim*), 83[130], 107, 110, 113, 120[+191, 192], 122[196], 129, 133, 134, 136[+217], 137, 138, 140–145, 150, 157, 158
Protogeometric period (see also Geometric) 34–36, 71, **153–154**
Proto-Sinaitic 68[107], 107, 110, 120, 121
Pseudo-hieroglyphic (Byblos) 53, **54**[+71], 57, 60[88], 70, **78**
Que 89[138]
Queen of Sheba 115[180], 118[186]
Raddana 15, 44, 45, 69
Radiometry 35, 66, 97, [98, 99], 101[160], 103–107, 109–114, 124, 130, 131, 153, 154, 156
Ramesses II 77
Ramesside 38, 57[81]
Ras Ibn Hani 53
Raybûn 96[+154], 99, **100–104**, 106, 107[+169], 109, 110, 111[+173], 113, 117, 121, 124, 125[+202], 126, 129, 130, 184
Rehov, Tel 35, 44, 83, 87
Revadim seal 71, 73, **83**[130],
rḥb^{n} (temple, Raybûn) 103[165]
Robin, Christian 9, 96, 116, 126[204], 184
Roche, Carole 9
Rosh Zayit, Horvat 36, 38, 83, 87
Royal city 51, 59, 62, 64, 65, 70, 89, 90, 92, 93, 151
Russian 100, 111
Ruweiseh arrowhead 24, 34
Saba(ean) (see also Queen of Sheba) 97, 100[157], 104, 106, 115, 118[+185], 128[209], 131, 132, 184
Sabaic see Sabaean
Safa 103[163]
Sam'al 65, 89[138], 90
Samaria (see also Israel) 59
Sardinia (see also Bosa, Nora) 33[29, 30], 68
Sarepta 53, 55[+73], 66
Sargon 115
Sasnuri **93–95**, 115
Saudi Arabia 128
Sawdâ', es- 96, 106, 108, [109], 113, 125, 126, 183
Seal (see also Revadim seal) 26, 87, 96, 111, 113, 114, 115[180], 125[202], 127, 128, 129, 130, 131[+212]
Sea of Galilee 67
Semitic see West Semitic
Sennacherib 115, 117
Septuagint 123[199]
Sérandour, Arnaud 9
Sfireh 144
Shalmaneser III 64, 94[+152], 95
Shamshi-Adad V 95
Sharon, Ilan 69[108]
Sheba see Saba and Queen of Sheba

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Sheshonq I 15[+9], 16, 17, 20, 22, [47], 66[103], 70, 73
Shishak see Sheshonq
Shrago, Pavel (Pasha) 9
Sinai 112[174]
Šipiṭbaal 15[10], 16, 17[+11], 20[+12], 23[15], 25, 27, 29, 30, 31[+25], 32[+28], 43[45], 45, 46[+50], 47[+53, 54], 48, 49[+58], 85, 86[135]
Solomon 17, 36[35], 66, 67, 86, 92[149], 118[186]
South Arabian / Southern Arabia see Arabia
Spatula (see also Byblos spatula) 54
Speshilov, Alina 9
Stern, Ephraim 66
Ṣubr 105, 106, 111
Suhis II 56[75]
Suhu 118
Syria(n) 53, 57, 59[85], 63, 64, 67, 69, 89[137], 90, 112[174], 153
– Northern Syria / north Syrian 38[+40], 43[47], 51[63], 57[80], 58, 59, 61[+91], 65, 66, 67, 69, 70, 75[+114], 76[+120], 90[139], 92[+148], 94[152]
Syrian Desert 112[174]
Syro-Anatolian see Syro-Hittite
Syro-Hittite 64[99], 77, **89–92**, 183
Taanach 79[+125], 80
Tabal 89[138]
Taitas 183
Tayinat, Tell (see also Patin) 79, 90
Tayma 131
Teixidor, Javier 9
Tekke 14[8], 15, 24, **34–36**, 38, 39, 43, 45, 48, [49], 52, 55[74], 68[+106], 69, 71, [72], 73, 121, 135, 153, 154, 157
Tel/Tell see under second word
Tel Aviv University 69[108]
Terqa 56, 94[152]
Thareani, Yifat 114[178]
Thera 137[218]
Third Intermediate Period 77
Tiglath-pileser I 62, 90, 118
Tiglath-pileser III 38, 49[58]
Tihâma 105[168]
Til Barsip 57, 63, 89[+138]
Transjordan(ian) (see also Jordan) 66, 123[199]
Tukulti-Ninurta II 56, 118
Tyana/Tyanide 140, 148[+245], 149
Tyre/Tyrian 66, 67
Tyrrhenian Sea 156
Ugarit(ic) (see also alphabetic cuneiform) 51[65], 53, 70, 89, 110, 122, 183
Ula, al- 131
United Monarchy 66
Unqi see Patin
Urartu 110
Vowel letters (Phrygian and Greek) 138[+222], 139[+225], 140, 147, 148[245], 149, 155, 158
Wadasatini 183
Wenamun 17, 47
West (Phoenician) see Phoenician westward expansion
West Semitic (occasionally "Semitic") 9, 11, 13–74 (*passim*), 76[120], 87, 96, 111, 113[176], 114, 115[180], 117, 119–122[+195, 197], 123[+200], 127, 129, 133–136, 138, 140, 142, 144, 145[+233], 150, 151, 154, 157, 158, 184
Wissmann, Hermann von 116[184]
Word divider 85, 139[228]
Yadin, Yigael 86, 153
Yahimilk 15[10], 17[+11], 20[+12], 23[15], 24, 28, 30, 31, 32[28], 46[+50], 47[+53, 54], 54

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Yakar, Jak 9
Yalâ 96[+154], **97–100**, 106, 107[+169], 109, 111[+173], 112, 113, 121, 122[194], 124, 125[+203], 126, 129, 130, 131
Yasur-Landau, Assaf 9
Yeha 126[204]
Yehimelek see Yahimilk
Yemen(i) 105[168], 107[+170], 111, 112, 128, 129, 130
Yiṯaʿʾamar Bayyin son of Sumuhuʿalî (Assyrian Itaʾamra?) 115[+181]
Zakarbaal 17, 47
Zakkur stele 83

LETTERS

alep / alif / alpha 23[+15], 24, 31[+26], 32, 33, 41, 104, 117, 125[+202], 126, 127, 136, 137, 141
bet / ba / beta 23, 24, 27, 30, 31, 32, 41, 46, 85, 86, 119, 141, 143
gimel / gim / gamma 23, 24, 41, 119[+187], 125, 126, 141
dalet / dal / delta 23, 24, 32[27], 41, 46, 85, 119, 126, 141, 142, 143[+230], 144
he / ha / epsilon 23, 24, 30, 32[27], 40[+44], 41, 47, 52[68], 71, 85, 126, 141, 142, 143[+230], 144, 145[+233], 155
waw / digamma 23[+15], 24, 26[17], 30, 31[+25], 32, 33[+29], 41, 43[+45], 46, 47, 52[68], 86, 141, 147
zayin / za / zeta 23, 24, 27[20], 30, 41, 46, 47[54], 52[68], 85, 141, 148[244]
ẓa 128[209]
ḥet / ḥa / eta 23, 24, 40, 41, 44, 104, 123[199], 126, 141, 143[+230], 144, 148[244]
ḫa 123[199], 126
ṭet / ṭa / theta 23, 24, 41, 128[209], 141, 148[244]
yod / ya / iota 23, 24, 41, 85, 111, 141, 143, 148[244]
kap / kaf / kappa 23, 24, 27[20], 41, 43[+45, 47], 46[51], 52[68], 138[222], 141, 142, 143[+230], 144, 145
lamed / lam / lambda 23, 24, 26[17], 41, 43, 55, 119[+187], 120[+191, 192], 121, 125, 136[+217], 141
mem / mim / mu 23, 24, 26[17], 27[20], 30, 31, 33, 39, 41, 43[+46], 44, 46, 47, 111, 119, 121, 125, 126, 136, 137, 141, 142, 143[+230], 144, 145[+233], 158
nun / nu 23[+15], 24, 26[17], 30, 31[+23], 33[+29], 39, 40, 41, 47, 119[+187], 120[+192], 121, 122[+194, 195], 141, 142, 143[+230], 144, 145, 155
samek / ksi 23, 24, 41, 43, 141, 148[244]
ʿayin / ʿayn / omicron 23, 24, 29, 41, 46[51], 52, 85, 119[+187], 120, 121, 123[199], 136, 137[+218], 141, 142, 144[232], 145[233]
ġayn 123[199]
pe / fa / pi 23, 24, 41, 119[+189], 141
ṣade / ṣad / san 23, 24, 26[17], 41, 43, 119, 125, 126, 128[209], 141, 148[244]
qop / qaf / qoppa 23, 24, 27[20], 30, 31[+25], 32, 33, 41, 47[+54], 119[+187], 141, 144, 145, 148[244]
resh / ra / rho 23, 24, 41, 111, 125, 126, 141, 143
shin / sigma 23, 24, 41, 119[+187], 120, 121, 136[+217], 141
taw / tau 23[+15], 24, 30, 31, 32[+28], 33[+29], 41, 52[68], 55, 71, 85, 86, 119[+187], 137, 141, 142[+229], 143, 144, 145
upsilon 138[222], 143[230], 147
psi 137
omega 137[218]

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